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ON THE DIMENSIONS OF MOLECULES.

By WALTER G. WOOLLCOMBE, M.A. (Oxon.), B.Sc. (Lond.).

(*Continued from page* 118.*)

WE shall use as our unit of work the milligramme-millimètre [mg. mm.]—that is, the work done in raising 1 milligramme through the distance of 1 millimetre against the attraction of the earth. Joule has shown that 424,000 mg. mms., that is the work done in raising 424,000 milligrammes through 1 millimetre, would, if transformed into heat energy, be capable of raising the mass of 1 milligramme of water through 1° Centigrade.

It is well known that the free surface of all liquids exerts a certain contractile force called the "surface tension" of the liquid, which gives rise to many familiar capillary phenomena. Thus, since the surface of water in a narrow tube is concave, its surface tension causes it to rise in the tube to a certain height above the level of the water in the vessel in which the tube is immersed. In the case of mercury, the surface is convex, therefore its surface tension causes the mercury in a narrow tube placed in a vessel containing the same liquid to be depressed below the exterior level. The values of the surface tension for different liquids can be easily determined by experiment. In the case of water it has been found to be equal to the weight of 16 milligrammes per millimetre of breadth; that is to say, in order to increase the area of a film of water by one square millimetre we shall have to do 16 mg. mms. of work. Thus the work done in stretching a film of water is numerically equal to sixteen times the number of square millimetres by which the area is increased. But at the same time the film is cooled on being stretched; and Sir W. Thomson* has shown, by the second law of thermo-dynamics, that, if we wish to keep the film at a constant temperature during the process, half as much energy again must be given to it in the form of heat as is employed in stretching it: therefore for every square millimetre added to the area of a film kept at a constant temperature, its energy increases by 24 mg. mms.

Suppose now we take a cubic millimetre of water at 0°C, and stretch it so that its area becomes 10,000,001 square millimetres (*i.e.*, its thickness very nearly 10^{-7} mm.), its temperature being kept constant, the energy spent in doing this would be equal to 240,000,000 mg. mms., which, on dividing by 424000, is seen to be equivalent to the amount of heat required to raise the mass of water more than 550°C. Long ere this the water would have volatilised, so that it is clearly impossible to draw out the film to so great a degree of thinness unless the surface tension is considerably reduced. On no molecular theory can we consider it possible that the surface tension should decrease as long as there are several molecules in the thickness, so that we are reduced to the conclusion that there can be only a few molecules in a thickness 10^{-7} mm. of water.†

Sir W. Thomson's second method is based upon the electrical attraction between zinc and copper plates. It is well known that when two insulated plates of these metals are brought into contact either directly or by means of a conducting wire, they assume a difference of potential, and exert in consequence an attractive force on each other. By means of an electrometer the value of this force can be calculated in absolute measure. It has been thus found that two plates, one of zinc the other of copper, each one square millimetre in area, when at a distance of 10^{-4} mm. attract each other with a force equal to the weight of 20 milligrammes. From this it is easily seen that the work done by the plates in coming together until the distance between them is 10^{-4} mm. is 20×10^{-4} mg. mms., or 2×10^{-3} mg. mms. Evidently the work done does not depend on the thickness of the plates. Now, suppose we have 5,001 plates of zinc and 5,000 plates of copper, each one square millimetre in area and 10^{-4} mm. in thickness. Let us arrange them alternately at a distance of 10^{-4} mm. apart from each other, bringing adjacent pairs of plates into momentary contact so as to produce the difference of potential. The work done in making this pile is evidently $2 \times 10^{-3} \times 10^{\times 4}$, or 20 mg. mms. The mass of the metal is 8 milligrammes very nearly: hence the amount of work done is 2·5 mg. mms. for each milligramme of metal. The specific heat of zinc and copper may be taken as $\frac{9}{100}$ths that of water: therefore to raise one milligramme of the metal one degree would require $\frac{9}{100} \times 424000$ mg. mms., or 38,160 mg. mms. of energy. The work done in building the above pile would, if transformed to heat, raise the mass of metal only $\frac{1}{15264}$th of a degree Centigrade.

But suppose we made, in a similar way to the above, a pile consisting of plates 10^{-7} mm. thick, the space between each plate being also 10^{-7} mm.

The force now between two adjacent plates would be equal to the weight of 20×10^{-6} milligrammes, and the work done by them would be a million times greater in this case than in the former. If converted to heat this energy would be

* Mr. Woollcombe desires to correct a slip in the seventh line of the second column of page 118. If r is the radius of the molecule πr^2 will evidently be the sum of the great circle areas of the molecules, not four times the sum as stated in the April number.

* "Proceedings of Royal Society," ix., p. 255.

† Professors Reinold and Rücker have made a series of very interesting experiments on soap films, by which they show that the surface tension of a film 10^{-4} mm. thick is the same as that of the liquid in mass, so that at this thickness there is no indication of an approach to the diameter of a molecule. "Phil. Trans.," 1877, 1881, 1883.

XUM

able to raise the temperature of the mass of metal 60° C. This amount of heat is greater than that produced by their chemical action when melted together to form brass. Thus we cannot reduce a plate of zinc or copper to the thickness of 10^{-7} mm. . . . . without making it cease to be zinc or copper as we know and handle them.

The five methods that have been mentioned tend to show that the diameter of a molecule is less than 2×10^{-6} mm. and greater than 10^{-7} mm.

In order to form a conception of the size of molecules, Sir W. Thomson bids us imagine a rain-drop as large as a pea to be magnified up to the size of the earth, each constituent molecule being magnified in the same proportion. The molecules would then be larger than small shot, but smaller than cricket-balls.

Knowing now the dimensions of molecules, and the density of a gas, we can calculate the absolute weight of a molecule of the gas. It is thus found that 144 trillions of hydrogen molecules weigh one milligramme.

"We are forbidden," says Sir W. Thomson, "by the modern theory of the conservation of energy to assume anything short of perfect elasticity of the ultimate molecules of matter." * If, however, the assumption we made at the beginning—viz., that the molecules are perfectly elastic spheres—be not granted, the above dimensions must be taken as applying to the distance between the centres of two contiguous molecules, each being enclosed in a spherical space beyond which its influence does not extend, and which is called by Clausius the "molecular volume." Whatever theory we adopt, we must allow that the molecules of a gas have time to vibrate a great number of times between two successive collisions, as it is to these vibrations that we owe the spectral lines characteristic of the gas in question. If we shorten the free paths by compressing the gas, the collisions become more frequent, and there are introduced many irregularities of vibration which cause the widening of the lines, with the natural vibration still acting as the keynote.

The actual motion of a molecule consists of three parts: (*a*) The motion of translation which is uniform between two collisions; (*b*) the motion of rotation which is constant for each molecule; and (*c*) the motion of the different parts of a molecule which is harmonic; which last possibly gives rise to the characteristic spectral lines of the gas. Clausius divides the total kinetic energy of a molecule into two parts—(i) the kinetic energy of translation; (ii) the kinetic energy of the parts of a molecule—and he assumes that the total kinetic energy bears a constant ratio to the kinetic energy of translation.† Let this constant ratio be signified by β; then if κ is the ratio of the specific heats at constant pressure and at constant volume, it is easy to show that

$$\beta = \frac{2}{3(\kappa - 1)}.$$

The value of β for permanent gases is 1·643. Now if the molecule consisted of a single atom, the kinetic energy of translation would be the total kinetic energy and β would be unity.

Putting β equal to unity in the above equation, we calculate $\kappa = 1{\cdot}67$. It is found by experiment that for mercury this is the value of the ratio of the specific heats, so that by this means we confirm what has been shown in other ways, viz., that the molecule of mercury consists of a single atom.

* This is evident; for if they were not perfectly elastic there would be a loss of motion at every collision, and they would eventually come to rest.

† It is only on the assumption of this constant ratio that we are able to measure the temperature of a gas by the mean kinetic energy of translation $\frac{1}{2}m\omega^2$.

THE DOMAIN OF MINUTE LIFE.

By Henry J. Slack, F.G.S., F.R.M.S.

THAT the domain of life is of far greater extent than was imagined a few years since, is one of the most wonderful results of recent investigation. When the opponents of what used to be called "spontaneous generation" asserted that what took place in infusions was the development of germs, either attached to the fermentible matter, such as hay, or introduced from the air; the reply of the other party was, first, that in their experiments they had destroyed or excluded any such germs, and in the second place that, if they did exist in the numbers required to account for the results, the air would be made thick and too dense to breathe. The microscopists who sided with these *abiogenesists*, or believers that new life was developed by putrefaction, thought themselves justified in denying the existence of more germs than they managed to see. Like all who had watched the dance of motes in the sunbeam, they knew that a multitude of small particles floated in the air, and that when it was still enough they gradually settled down as fine dust. The microscope showed that this dust was mainly composed of a variety of dead substances, organic and mineral, minute fragments of woollen, linen, cotton, and other clothing stuffs, together with particles rubbed off all the other substances in general use, and also whatever the wind picked up from plants, animals, or the soil. In towns and near dwellings these things abounded, and became scarcer and scarcer in elevated districts remote from all the operations of man. Some small eggs and germs the spontaneous generationists did see, but not nearly enough to account for the multitudinous life that appeared when any of the air-borne dust was supplied with water containing food material that had been sterilised and was kept at a moderate temperature.

The last important defender of the spontaneous generation opinions was F. A. Pouchet, member of the French Institute, and a painstaking man. He called his theory "heterogeny," by which he meant a genesis, or new birth, related to, but differing from, the processes of ordinary propagation.

If a wisp of hay or other fermentible material is steeped in water, and kept in a warm place, swarms of minute rods and serpentine wriggling objects soon appear. Their life is a brief one, but they divide and multiply with great rapidity, and, as they die, they get felted together in a pellicle, or thin skin. Pouchet thought this formation, which he called a "proligerous pellicle," was a real mother of the higher creatures that appeared quickly after its formation.

The notion that putrefaction was an actual cause of new life is a very old one, and easily arose in prescientific times; for, did not maggots multiply as meat decomposed? Under the microscope it is seen that as animal and vegetable matter rots away, swarms of ferments come into existence. For example, in a drop of water, the flesh of a dead water-flea was noticed in commotion while the writer was engaged on this paper. Thousands of U-shaped vibrions were living upon it; all were in brisk motion, straightening and bending their bodies with whip-like flicks. They were a company of scavengers, sweetening the water by a chemical process necessary for their own nutrition. Our rivers and ponds would become factories of deadly poisons, and all the earth's soil would be contaminated, if inexpressible myriads of minute plants and animals did not attack dead organic matter, and cause its elements to enter into new and useful

combinations. If we find thousands of such little ferments at work upon a fragment of matter no bigger than a full stop of this print, what must be the numbers in operation when tons upon tons are dealt with in the contents of our sewers, in the manures we put on our fields, and in the vast multitudes of human and other bodies that perish on land or in sea.

The ancients fancied that crocodiles sprang from the mud of the Nile, and Pouchet was not behind them in magnifying the powers of putrefaction. If no cows or crocodiles now emerged out of a parental slime, it was because we lived in quieter geologic times, in which change was slow and gradual, and all creation did not simultaneously go to smash and run to putrescence on a world-wide scale.

Lest it should be supposed that the above is in the least a caricature of Pouchet's philosophy, the following literal translation of a passage in his "Heterogenie" is submitted to the reader: "Our internal sense," he exclaims, "should persuade us that if at successive epochs it has pleased the Deity to establish the laws which preside over organisation, these same laws were not abrogated at the moment of last production; they continue subjecting everything to the immovable destiny emanating from the Supreme Wisdom; but only their manifestations do not reach the proportions of ancient times, they are diminished, like many other telluric phenomena. We have no longer in fermentation those immense masses of dead matter that resulted from so many cataclysms and funerals of animals; so, instead of the gigantic races that emerged from the midst of the agitated elements, we only see the production of the lowest efforts at organisation."

This was published in 1859, and it was some excuse for the very erroneous estimation of the extent to which germs exist and were diffused, that the microscopes and modes of investigation employed by Pouchet and others of similar ways of thinking could not possibly have enabled them to see anything at all in many cases where the present means and appliances, skilfully used, show a great abundance of minute life. Those who, with some width of interpretation, accepted Harvey's saying, *omne vivum ex ovo*—"every live thing from an egg"—knew that more spores and germs than could be discerned with the older instruments must be widely diffused to account for the abundance of life that sprang into existence without visible cause; but experimental proof could only be obtained with improved means and greater skill in their application. Few persons had any adequate conception of the care and precaution necessary to make anything absolutely clean and free from germs, and then to keep it from contamination. Air, for example, that seemed quite free from suspended particles, revealed their existence to Tyndall's electric sunbeam, and, so long as the faintest shimmer could be observed, life-germs were probably amongst the minutest motes. But when the searching light beams gave a purely negative result, proving that all the particles had subsided, the air could be allowed to remain in contact with fermentible matter that had been sterilised without any form of life appearing.

Pasteur had previously found that the complete exclusion of germs required extreme precautions. Thus, when he had boiled his infusion in small flasks, he drew out their necks in a blow-pipe flame, twisted them about, and finally sealed them hermetically while the contents were quite hot. No life appeared under such conditions, but if a flask's neck was broken and a puff of the air of ordinary rooms permitted to enter, the usual swarm of bacteria and similar things soon appeared. If he took one of his flasks into the open country and then admitted air the germs were fewer, and on high mountains sometimes none were found. In these experiments two possible sources of error were guarded against. Before he broke a piece off the neck of his bottle he made it hot in a spirit lamp, and he likewise held in the flame the nippers he used. The neck of the flask was then sealed up by heat. If no life appeared after the flask had been kept for some time in his laboratory, it soon became manifest if the neck was broken.

It may be interesting to many readers who are not microscopists to explain why many germs and living things are so difficult, and some impossible to see, except when they swarm and form a cloud. In the first place, there is a limit on the ground of extreme minuteness, and no microscopes magnify billions of times as silly paragraphs in newspapers often assert. In 1878 Dr. Dallinger succeeded in seeing that a very minute bacterium (*B. termo*) about $\frac{1}{10000}$th of an inch long was furnished with two whips, one at each end. By two series of measurements—50 with a 24th-inch objective, and 50 with a 35th-inch—a mean was obtained showing the whip to be only $\frac{1}{204700}$th of an inch across. The highest magnification in this case was 4,000 linear.

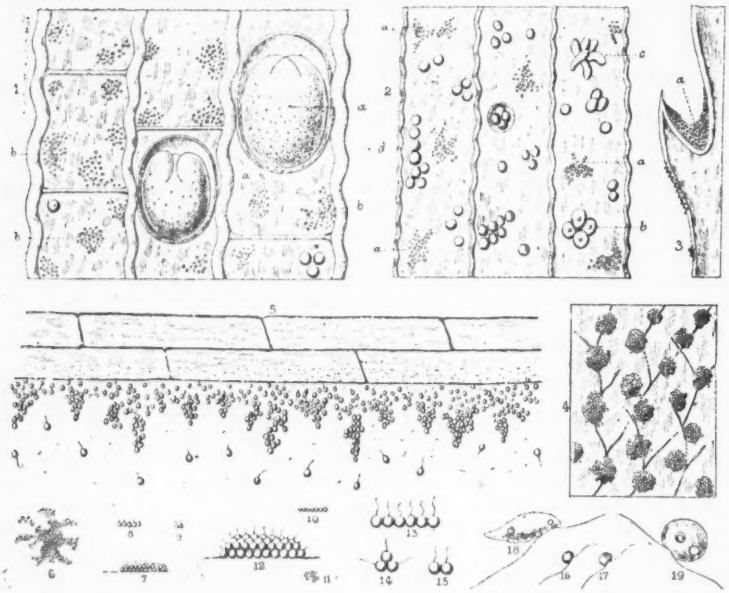
Mr. Saville Kent, in the frontispiece of the plate volume of his great work on Infusoria, gives an astounding representation of a collared monad, an oval creature with a stalk foot at one end, and a delicate wineglass-shaped collar of sarcode at the other, with a whip vibrating in the midst of it. He obtained this view with a magnification of 8,000 linear, and the top of the whip looks no bigger than a fine hair. As applied to live objects, no more important success has been obtained than the display of fine whips by Dr. Dallinger, because the things are not only of excessive tenuity but their refractive power differs so little from that of water that it needs not only a fine objective but the nicest illumination to render them visible. It is easy to see a drop of water when it is isolated, but it cannot be recognised when it is surrounded on all sides by others like itself. Now a great many of the things a microscopist wants to see are very transparent, and differ exceedingly little from water in their action upon light. This is very often a greater hindrance to seeing them than the smallness of their size.

The illustrations appended to this paper are taken from Saville Kent's work, and they give an excellent notion of what can be seen by a skilled observer under favourable conditions. Fig. 1 represents a fragment of hay fibre after six hours' maceration; *a a*, are encysted *Vorticellæ*, creatures that used to be called "Bell animalcules," being something like those articles supported on slender stalks. Like many other infusoria, they sometimes retire from active life, and do themselves up in a cyst or bag—a sort of taking the veil, though not for ever; *b b b*, are microspores of a lively little thing—*Heteromita lens*—that dashes about in a surprising way, often anchoring itself with one whip that has a power of adhesion, and jerking itself vigorously with the other. Fig. 2 shows a hay fibre wetted, and immediately examined; *a a a*, microspores of *H. lens*; *b*, macrospores of *Oikomonas mutabilis*, a whip-swimming thing assuming various shapes; *c*, macrospores of *Heteronema caudata*; *d*, unknown × 800. Fig. 3 shows a fragment of hay fibre, with *a*, a mass of microspores caught in a crevice of the serration. Fig. 4 globular spore masses of *H. lens*. Fig. 5 piece of hay fibre after two weeks' maceration, showing bacterial film and monad spores in grape-like clusters; minute monads developed from these spores, mixed with vibrios and bacteria swimming beneath × 800. Figs. 6–17, *Heteromita lens*; 6 and 7, microspores × 800; 8 and 9, free-swimming monadiform germs developed from the microspores × 800; 12 to 15 × 2,500; 16 and 17, succeeding biflagellate condition of same germ × 800.

The smaller of these objects are difficult to detect, and

some could not have been seen until comparatively recent improvements in the objectives and in the apparatus for illuminating. The macrospores spoken of are large spores by comparison with the much smaller microspores. Many of the lower organisms produce the two kinds, the smallest resulting from a coalescence of two or more adult forms, followed by a series of internal changes, and finally a break-up and dispersion of countless germs.

Although the air in all places near the earth and its inhabitants contains swarms of germs, it is not their birth-place, and they are not developed in it. Much of their life history is unknown, but perhaps a majority of some kinds



are raised on grasses and various other plants, and others upon animal or vegetable matter in a decomposing state. Some spores resist wetting, and are preserved in a quiescent state until suitable conditions for their development occur. Millions must, however, perish without a chance of growth. This is also the case with a great many germs, eggs, spores, and seeds of higher forms. Are they, therefore, wasted? Certainly not; besides supplying food for other creatures, their formation brings, in the aggregate, myriads upon myriads of tons of inorganic matter within the organic circle; and if we knew the whole scheme of creation we should probably find that the transitions from the inorganic to the organic and back again are the results of a widely operating law.

Amongst the places where quiescent life may be found, are the rain-gutters attached to buildings. In dry weather many spores of infusoria, some rotifers' and other eggs, and minute worms, such as the so-called paste eels, pass into a resting state, and revive when warmth and moisture are supplied. They are protected against a fatal degree of desiccation by a gelatinous envelope, and as long as their structure remains uninjured they can be brought to life again. Lord Godolphin Osborne is very skilful in managing them. He dries them amidst the sand and other particles in which they are found, and they revive after long intervals, as the old observers said. The way to fail is to isolate the little animals from their natural surroundings and dry them up quickly. Pouchet must have done this when his experiments led him to think the resurrection of dried rotifers was a mistake, and all that happened was their swelling out as they absorbed water, and then making some purely mechanical movements. In the winter time, ice taken from water-butts or ponds will often be found to contain some of the larger infusoria, with rotifers and green vegetable monads. The accounts given by early observers of the common rotifer (*R. vulgaris*) are very amusing, and especially where the creature had become torpid in ice and revived under the microscope. It is always a striking sight to see a passive potato-shaped thing push out a telescopic tail at one end, and what look like a pair of wheels at the other, in brisk rotation.

Our business now is not, however, to describe rotifers, but to mention ice as a thing to be examined for minute life. Snow and rain also bring down all manner of particles floating in the air, a multitude of germs included. We live, in fact, in the midst of vast populations, of which no one until recently imagined the existence. They are on us, in us, and around us. On the whole they benefit us, but we are apt to think only of those that produce disease. Shakespeare's Death, who

> Comes at last, and with a little pin
> Bores through his castle wall, and—farewell king,

has its unpoetical, but sadly practical, illustration in the little rods, billions of which would not reach the dimensions of the "little pin," but which can bore through life's castle walls, and decimate a population that disregards the laws of health.

If we could pay a Jules Verne visit to the depths of the sea, we should find it swarming with minute life, and in no wise destitute of the larger kinds. Fish have been brought up from a depth of about $3\frac{1}{2}$ miles, enjoying voracious lives under a pressure of some $3\frac{1}{2}$ tons per square inch. When Dr. Wallich, in 1862, first presented the scientific world with a starfish from a depth of 1,260 fathoms, he was met with great doubts, and philosophers of eminence, Faraday among them, could not believe that any animal of so delicate a structure could live under such immense pressure. Now it has been placed beyond doubt that the great deeps are inhabited, and that many of the things that dwell therein illuminate its darkness with their phosphorescent light. The same law of the higher being dependent upon the lower is true wherever various forms of life exist, and when the higher perish, the lower bring their myriads to the work of disintegration, preparing for another turn of the great Nature wheel.

THE MASSES, BRIGHTNESS, AND DISTANCES OF DOUBLE STARS.

By W. H. S. Monck.

DOUBLE stars in the proper sense of the word were unknown until after the invention of the telescope, for the simple reason that they are too close to be distinguished by the naked eye. This fact is easily explained. The parallax of the nearest fixed star yet measured, α Centauri, is about three-fourths of a second—in other words, a star situated as far from

α Centauri as the earth is from the sun would appear under the most favourable circumstances at the distance of three-fourths of a second from the principal star. Even at the distance of Neptune from the principal star, such a satellite could not be distinguished by the naked eye. The telescope, however, was not long in use before some of the more prominent double stars were detected, and fresh discoveries have been made with every increase of optical power and with every advance in the carefulness and skill of the observer. Double stars are now to be reckoned rather by tens of thousands than by thousands, and the cry is still they come.

It is of course possible that double stars may consist of two stars at very different distances from us but nearly in the same direction; but it has long been known that in a great many instances the component pair are physically connected. In the first place double stars are far too numerous to be accounted for by chance, and their distribution is quite different from what it should be on the doctrine of chances—small angular distances being relatively much more frequent than large ones. Then we have to notice the large number of pairs in which the components are alike both in brightness and colour—in which case, if their distances from us are materially different, there must be an almost exact compensation in the greater quantity of light given by the remoter star. A more singular fact noticed by Sir J. Herschel is that no green or blue star of a decided hue has hitherto been found unassociated with a companion brighter than itself.* As there are probably not less than 200 of these green or blue stars known, the chances against each of them being accidentally almost in a line with a brighter star are enormous. Many pairs of double stars are characterised by common proper motion, and finally orbits have been computed for over forty of them. Some of these orbits have been computed from observing a small portion of the star's path, and may therefore prove seriously in error; but in other cases the satellite has completed more than an entire revolution round its primary since it was first observed, and there can be no doubt as to its orbit—or rather as to its apparent orbit traced on the celestial sphere.

Double stars are probably among our nearest neighbours; for it is evident that any double star might be removed to such a distance that the components would be no longer separable even by our most powerful telescopes, and where the contrast of light between the pair is great the smaller star might disappear before reaching this distance. The greater part of the stars whose parallaxes have been measured (at least where the measurements are at all reliable) are double stars,† and some of those which are not known to be double are suspected to be so in consequence of their motions. Sirius belonged to this latter category thirty years ago. Procyon does so still; while recent measurements give increased distances for almost all the bright stars which are not known or suspected to be double. There are thus grounds for conjecturing that duplicity (or multiplicity) is the ordinary condition of stars, and that single detached stars are comparatively rare, the ordinary single star being a very remote or very close double or a double with one very faint component. At the same time it is to be noted that even double stars with known orbits may be extremely remote provided that their masses are sufficiently large. We might substitute for any given double star a pair of eight times the mass at double the distance, or one of one thousand times the mass at ten times the distance, without affecting the visible orbit in the least—the actual distance between the components being, of course, increased in the same ratio as the distance of the pair from the earth. In general, we might replace any binary system by one at n times the distance with n^3 times the mass without disturbing the orbit. Where the parallax has not been ascertained the only improbability is that the mass should be very much larger or smaller than that of the sun, or in the case of a pair with known proper motion that they should move so very fast or so very slowly as the assumed distance might imply. In general, however, if we assume a very small mass the parallax would be measurable, and where parallax-measurements have been tried without a satisfactory result, the notion of a very small mass and near distance must be given up. On the other hand, there is no reason to think that the average motion of stars perpendicular to the line of sight is greater than their average velocity in that line, and as 100 miles per second is a velocity rarely observed in the line of sight, we may fairly object to assuming a mass for a binary star, which supposes it to be moving with more than that velocity in a direction perpendicular to the line of sight.

It is singular, however, that the brightness of a double star likewise fails to give us any indication of its distance when the orbit is known. Supposing that the star was replaced by another exactly similar in density and illumination of surface at double the distance and with eight times the mass, the light of the star as well as the orbit would remain unaltered: for the illuminated surface would be four times as large as before, and this surface at double the former distance would give the same amount of light. And if we place a star of n^3 times the mass at n times the distance, the surface will be increased in the proportion of n^2 to 1, and the light will still be constant. I assume, of course, that the medium which transmits light is perfectly transparent. If any light was lost on the way the result would be different. But with a perfectly transparent medium neither the orbit of the star nor its brightness (as photometrically determined) affords any indication either of its mass or of its distance.

But when both distance and orbit are known, we can determine the mass; and in a few instances both are known approximately. The indications thus afforded lead us to regard the mass of the sun as a kind of standard from which very violent departures on either side are not to be expected. Sirius was supposed to be much larger than the sun, but the recent determination of Dr. Gill giving its relative parallax at 0·38″ (or its absolute parallax at, say, 0·40″) reduces the combined masses of the star and its satellite to about six times that of the sun. α Centauri is probably larger than the sun but smaller than Sirius. A double star in Eridanus has lately been computed to be almost exactly equal to the sun. 61 Cygni seems to be smaller, but doubts still exist as to the orbit of this double star. A recent determination makes the double star 85 Pegasi eleven times as large as the sun, but the parallax of this star is doubtful. The stars in question are too few to place much reliance on the results derived from them, but taking the sun as a standard, and assuming the mass of each pair of double stars to be equal to his, it would seem that orbits for most of the nearer double stars have been

* This statement must now be somewhat modified. No companions have as yet been discovered to, for example, the following decidedly blue stars: γ Tucanæ, θ Eridani, ν Puppis, ϵ Pavonis.—[EDITOR.]

† This statement needs some qualification. Out of the 34 stars whose parallaxes are given in Young's "General Astronomy" (and these include all the best determined parallaxes), only 10 can be considered as double stars—a large proportion, no doubt, and the fact is well worthy of notice. Amongst the ten doubles I do not include α Lyra, but I have included 61 Cygni. In the case of 61 Cygni, the binary connection of the two stars is at least doubtful. [EDITOR.]

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already computed, and that the parallaxes of the double stars recently computed do not average much more than $\frac{1}{50}$th of a second, which distance we have reached without exhausting more than fifty double stars out of 10,000! A parallax of $\frac{1}{50}$th of a second corresponds to about 10,000,000 times the distance of the sun from the earth. What must the average distances of the remaining double stars be? And then what is the average distance of the single stars, if the majority of them are really double stars which have escaped detection on account of their remoteness? There are double stars whose physical connection appears certain, and whose angular distance is not very great, but which cannot be said with certainty to have moved through an angle of one degree in a century. In fact, it may be said of the majority of double stars that the angular motion is so slow, that the observed change in twenty years is within the limits of error. A longer period of observation is necessary to ascertain whether there is any motion at all.

One fact which is clearly proved by double stars is that the light of a star is by no means proportioned to its mass. From the masses already mentioned, and the fact that Sirius is twice as far from us as α Centauri, it is plain that Sirius is not only the brighter star, but brighter relatively to his mass. The same result would arise from comparing Sirius with the double stars in Pegasus and Eridanus previously referred to; but Sirius is far surpassed in intrinsic brilliancy by the fine double star γ Leonis. With a mean angular distance of only two seconds between the components, the satellite of γ Leonis takes 400 years to perform its revolution round the principal star. To reduce the light of Sirius to that of γ Leonis we should have to remove the former star to about five times his present distance. A satellite at the angular distance of 2″ would then revolve round Sirius in about seventy years. It is true, no doubt, that if Sirius was removed till his light became equal to that of γ Leonis, the two stars might not be at the same distance from us; but I have already pointed out that γ Leonis might be replaced by a star of the same intrinsic brightness at the new distance of Sirius without affecting its orbit. The mass of such a star would be so much less than that of Sirius that its satellite would require 400 years to perform the revolution which the Sirian satellite accomplished in 70, yet the light of both stars would be equal. Again, if Sirius was removed till his light became equal to that of Castor, a satellite at the same angular distance as Castor's satellite would revolve in about 300 years. Replace Castor by a star of the same intrinsic brightness at this distance, and its satellite will require 1,000 years to perform the same orbit. γ Leonis and Castor are thus far brighter relatively to their masses than Sirius, whatever the explanation of this fact may be. It is proved by the orbits of the stars and the amount of light given by them as measured by the photometer, and is independent of the actual masses of any of them. Between the extremes hitherto observed, γ Leonis and 61 Cygni, the proportion probably exceeds 100 to 1. If composed of similar materials the contrast between them is similar to that of the sun in the zenith and the sun when he almost touches the horizon.

That one star may be composed of lighter materials than another seems highly probable, but the contrast of starlight (which will no doubt become greater when we have a greater number of stars to compare) indicates, I think, that the light of stars varies in every possible degree from very intense illumination to absolute blackness. In the solar system the sun is usually regarded as the only luminous body, but there are reasons for thinking that comets which are permanently attached to this system, and perhaps the larger planets, give some light of their own. Dark bodies in space seem equally probable. Their presence is indicated by the Algol type of variable stars; and some comets and meteors which have visited us appear to have been pursuing paths that must eventually carry them into the region of the stars. This difference in luminosity affords a further reason why the nearest stars are not always the brightest, and *vice versâ*; but the results hitherto obtained do not lead us to look for very great differences in mass between different stars, and when a faint star proves to be a comparatively near one, its luminosity is probably low.

Some of these inferences, however, would be much shaken if Sir Robert Ball's determination of the parallax of the double star 6 B Cygni should prove correct. The angular distance of this pair is little more than half that of 61 Cygni, while the angular motion is five times as slow. Sir Robert Ball makes the parallax of 6 and 61 nearly the same, in which case, as 61 Cygni is certainly smaller than the sun, the mass of 6 Cygni should rather be compared to that of Jupiter than of our principal luminary. If the mass of 6 Cygni is equal to that of the sun, its parallax does not probably exceed $\frac{1}{25}$ of a second. Professor Asaph Hall, however, finds the parallax of 6 Cygni insensible, and further observations are necessary to decide between the two astronomers. In the meantime I may point to this as one of the most interesting of our double stars. The distance between the components is only about 10 seconds, but at the present rate of motion it will require 5,000 years to complete a revolution. Its considerable proper motion is rather favourable to Ball's parallax, and as it is nearly as bright as 61 Cygni, it is clear that, assuming the distance of both stars to be the same, its intrinsic brightness must stand very high—higher, perhaps, than even that of γ Leonis.

It may be desirable, in conclusion, to add a word as to the orbit of a double star. All that can be observed is the orbit traced on the celestial sphere, which may be regarded as lying in a plane at right angles to the line of sight. But this orbit seldom satisfies the requirements of the law of gravitation, and we therefore assume that the true orbit is in a different plane; in general there are two planes (making equal angles with the line of sight) which fulfil the required condition. The law of gravity is thus assumed not proved, and the proof of it must depend on careful spectroscopic observations of the velocity of the star in the line of sight at different portions of its orbit. For some double stars, orbits differing considerably from each other have been computed by different writers. For Sirius I have adopted Colbert's. Mr. Mann's orbit would make the mass of the star considerably greater, and its brilliancy considerably less.

THE HEAD OF A BEE.

By R. Camper Day.

BEES have always been popular, and the reason is that the most superficial study of their habit of life reveals a succession of marvels of an obvious and striking kind. Everybody has observed their untiring industry, the excitement of their swarming-time, their accurately constructed combs, their diaphanous gauzy wings, and the pellets of pollen packed on their hind-legs. Those who keep bees in their gardens and watch them carefully are acquainted with many less obvious but still well-known wonders. Among these may be reckoned the ventilation of the hive by the "fanning bees," who pass currents of air from one to another by agitating their wings; the singular circumstances attending the bridal of the queen-bee; the

means by which a grub or larva which in the ordinary course would turn into a worker can be made to turn into that very different insect, a queen; the combats of rival queens; and the autumnal slaying of the drones. But the greatest wonders of all are reserved for those who have time and patience to study the anatomy of the bee with the aid of the microscope. Under the lens every part of the insect is found to be full of agreeable surprises. Take, as a few examples, the pockets under the abdomen in which the wax is formed; the beautiful row of little hooklets, by which the posterior wing is hooked to the anterior wing when the bee is flying; the remarkable construction of the honey-sac, or extra stomach; and the row of little holes or "spiracles" on each side of the body through which the bee breathes. Or take, as a more complete instance, the fore-leg, furnished as it is with a whole armoury of useful contrivances. At its tip there is the foot, with a sucker (*pulvillus*) for clinging to smooth surfaces, and hooks (*anguiculi*) for clinging to rough surfaces; higher up is an eye-brush of stiff bristles for sweeping dust and pollen from the creature's eyes; and higher still, in the elbow-joint (if the expression may be allowed), is a circular notch called the antenna-scraper, which cleans the antenna with one movement of the leg. Those who would go deeply into the whole subject are recommended to consult Mr. Cheshire's accurate and appreciative book on bees. In this paper I propose only to deal with what is perhaps the most interesting portion of the insect, namely, the head.

In shape and general appearance it resembles the pip of a ripe apple, the point of the pip corresponding to the bee's tongue; but whereas the pip is bright and shiny, the colour of the bee's head is mostly obscured by a fluffy down. The first and second of the illustrations to this article are the front and top views respectively of the head of a Polish bee, from the drawings of Girdwoyn. The Polish bee is a variety of the same species (*Apis mellifica*) to which the common English bee belongs, and resembles the English bee very closely. In all essential points the two varieties are identical.

The head, body, and limbs of a bee, it should be premised, are encased in a hard and horny shell, composed of a substance called chitine, the skeleton of the bee, like that of the lobster, being external; and this external armour is strengthened in the head by certain internal girders of the same substance connecting the front with the back. Of the interior of the head, however, the brain, the so-called salivary glands, and other matters of interest, I have not space to speak, and I must pass on to the consideration of the external features, which can be easily recognised and examined by any one possessing a pocket lens.

Of these the most salient and remarkable are three in number—the antennæ, the eyes, and the mouth.

A glance at the first illustration will show that each antenna consists of two parts. The portion nearest the head is a rigid rod, known as the "scape," articulated into the head by a ball and socket-joint which admits of free movement in any direction, and the remainder is a flexible chain called the "flagellum," made up of eleven short portions or joints. This jointed portion, or flagellum, is perhaps more important to the bee than any other piece of its anatomy. Examined under the microscope, it is seen to be studded with innumerable minute organs. It possesses two kinds of non-sensory hairs unprovided with nerves, and two kinds of tactile or feeling hairs. In virtue of these latter, which are extremely numerous, the antenna is a wonderfully delicate organ of touch, and until recently its function was supposed to be purely tactile. But a closer inspection has revealed the existence of at least two other kinds of organs besides hairs on the flagellum. One of them is a tiny elliptical disc, found in large numbers on every joint; the number has been estimated at 2,400 in a single antenna of a worker, but the drone or male bee rejoices in a multitude which probably numbers no less than 37,800. The function of these discs is disputed. Mr. Cheshire calls them "smell hollows." It has been proved that bees do possess the sense of smell in their antennæ, and it is highly probable that the faculty resides in these elliptical discs. However, there is, as I have said, yet another organ on the flagellum, a minute cup-like depression, which has also given rise to dissension among the experts, and which might possibly be the organ of smell. Every bee has a considerable number of these cup-like depressions, though they are not nearly so abundant as the so-called "smell hollows." Mr. Cheshire, with much plausibility, argues

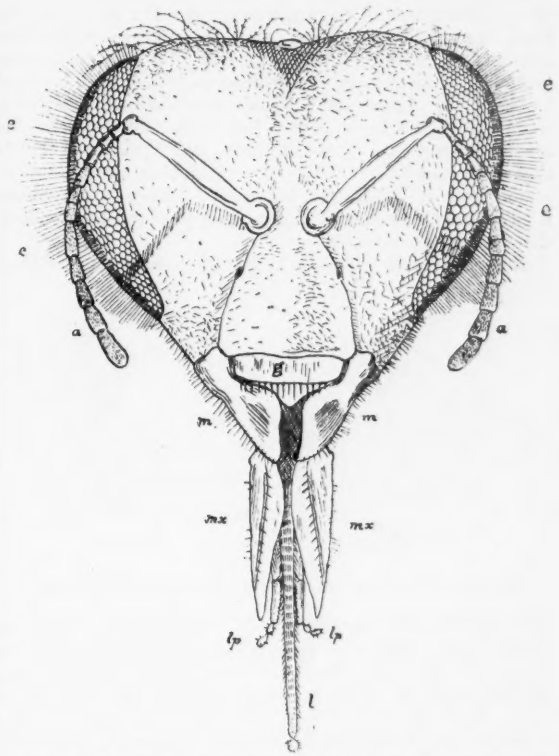

FIG. 1.—FRONT VIEW OF THE HEAD OF THE POLISH BEE.

them to be organs of hearing. But can bees hear? Sir J. Lubbock has conclusively shown them to be deaf to ordinary noises such as are produced by tuning forks, and the human voice. But there are noises, and loud noises too, which are totally inaudible to human ears. Many readers of KNOWLEDGE have doubtless heard of the little whistles invented by Mr. F. Galton for the purpose of testing the range of hearing in the lower animals. He has one of these whistles attached to the handle of his walking-stick, and blows it by means of a small indiarubber bag held in his hand. Human ears can detect no sound when it is blown, but all the small dogs within range instantly look round. The reason, of course, is that human ears are adapted to the appreciation of a limited range of sounds, those, namely, which are caused by vibrations not exceeding 38,000 in a second. Now there is reason to believe that bees, like other small animals, are perfectly sensible to sounds of a very high pitch. All bee-keepers are aware

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that at swarming time the queen bee emits a peculiar shrill piping note which produces a remarkable effect upon the bees. When they perceive it they seem abashed and frightened, as F. Huber first pointed out. This note is pitched so high as to be almost beyond our ken. When that mortal enemy of bees, the Death's Head moth, invades a hive, it utters an imitation of the sound, and thereby disarms the bees as completely as if by the voice of their own queen. It has been suggested that what we hear when the queen or Death's Head pipes, is only one of several notes, the others being beyond our range. In any case the facts seem to show that bees possess the power of hearing, and, if the sense is not located in the cup-like depressions above referred to, it is difficult to know where to look for it.

It may be taken as proved that the antennæ discharge the triple function of fingers, noses, and ears; and it is far from improbable that they may be endowed with other senses of the nature of which we can only have a dim conception—senses akin to that which enables the male bombyx to detect the presence of a female at a distance amounting to miles. Enough, however, has been said to show the sensory importance of the antennæ, and I may add that if anyone is uncertain on this point his doubts would soon be set at rest if he should happen to witness the extreme distress and helplessness of a bee which has been accidentally deprived of those organs.

Bees have five eyes, the two large compound eyes (e, e)

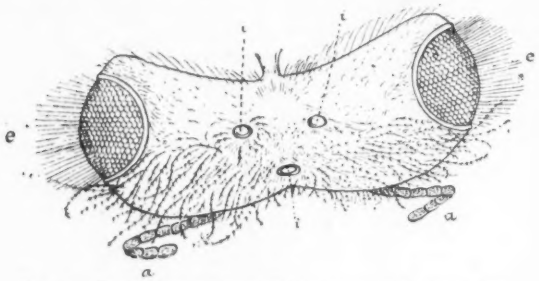

FIG. 2.—TOP VIEW OF THE HEAD OF THE POLISH BEE.

and the three little ocelli (i, i, i). The compound eyes are made up of a number of minute hexagonal facets (the queen has 5,000 on each side, the worker 6,000, and the drone 12,000), each of which is connected with the brain by its own chain of nerve fibres. Each facet is undoubtedly a separate eye conveying a different picture to the brain; it is very difficult to understand exactly what kind of vision a bee enjoys with these multitudinous rigid eyes so very unlike our own. One thing is pretty certain, that it sees in all directions at once; but whether with its many eyes it is conscious of a single complete picture, just as we with our two eyes see things singly, or whether it merely sees a kind of mosaic, a patchwork of many minute but separate pictures, is open to doubt. In the three ocelli on the top of the head the lenses are highly convex, and they are probably used for short-distance vision only.

The complicated apparatus with which the mouth is furnished presents a somewhat formidable appearance in our illustration, but it is not difficult to gain a clear idea of its nature and functions. The flap marked g (fig. 1) is the upper lip; the mentum or lower lip is hidden in the drawing by the mandibles and other mouth-parts. The strong mandibles m m, one on each side of the mouth, are the true jaws of the insect. They are short as compared with the other mouth-parts, but they have sharp edges and are worked by powerful muscles. With these jaws, opening and shutting sideways, the bee does its biting. But it will be seen that there are five other organs projecting beyond the mandibles, all five of which are attached to the mentum or lower lip. What is the function of this quintette? They form, of course, the apparatus by means of which the bee sucks up honey from flowers; but at first sight it is far from clear why so formidable a collection of tools should be required for so simple a function. Let us examine the tools more closely. Midmost of all is the long, lissom, and hairy ligula or tongue l; on either side of this we find the two labial palpi, $l\,p$; and again, outside these, the two maxillæ, $m\,x$. We will leave the tongue for the present, and concentrate our attention on the labial palpi and the maxillæ. These, like the mandibles, open and shut sideways. In fig. 3 two short pieces cut from the maxillæ are shown in perspective; and in fig. 4 we have two similar pieces cut from the labial palpi. The maxillæ, it will be seen, are almost semi-cylindrical in shape. They consist of a thickish portion, t, and a thin blade known as the hyaline plate, $h\,p$. Now, when the maxillæ are closed by the insect, the two hyaline plates overlap, as shown in section in fig. 5. The plate which happens to be outermost is stopped by the row of bristles h, and the edge of the other plate fits neatly into the notch n (fig. 3). In a very similar manner the two labial palpi are fitted together behind the maxillæ, so that when all four are closed they form a tube with the tongue in the middle (fig. 5). But observe that when they are closed the tube is not necessarily complete, for there is left a triangular opening in the space between the mandibles and the upper lip (g). But the edge of the upper lip is elastic and extensible, and when the bee wishes to use the tube it protrudes this edge so as to cover the vacant space. Here, then, we have a complete tube; but, given the tube, how can a bee use it for suction? Bees do not breathe through their mouths, and their stomachs have no muscles which could act suctorially. The suction is done partly by the tongue, which can be moved up and down inside the tube, and partly by the tube itself, which is capable of alternate expansion and contraction. In fig. 5 the palpi are shown touching the maxillæ, but the point at which they touch can be varied at the will of the insect. It may be at the two extremities $a\,a$ of the maxillæ, or it may be at the two ridges $r\,r$. In the former case the space inside the tube is considerably larger than in the latter; and by the alternate contraction and expansion the nectar of flowers can be forced up into the mouth, where it is converted into honey by the so-called salivary glands.

FIG. 3. MAXILLÆ.

FIG. 4.—LABIAL PALPI.

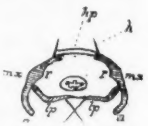

FIG. 5.—CROSS SECTION OF TONGUE PARTS.

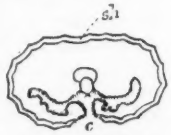

FIG. 6.—CROSS SECTION THROUGH TONGUE.

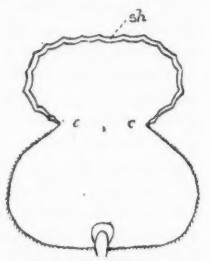

FIG. 7.—TONGUE DISTENDED.

But the bee can only suck with this tube when the quantity of nectar to be drawn up is considerable. When the quantity is very minute the bee could no more suck it up through the tube than a man could suck up a drop of water through a gun-barrel. In such a case, or if

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PHOTOPHANE, 846 OLD KENT ROAD, LONDON.

THE GREAT NEBULA IN ORION.

Enlarged about four diameters from an Original Negative taken by Mr. Isaac Roberts, with an exposure of 3½ hours, on 4th February, 1889.

the nectar is out of reach of the tube, the ligula or tongue is brought into play. Now the tongue, it should be noted, can be used in two distinct ways, either for lapping in dog-fashion or for suction; for, as figure 6 makes plain, the tongue is itself a tube, or quasi-tube. The figure referred to is a cross-section through the tongue. It will be seen that the organ is encased in a stiff sheath, *sh* (it is covered with hairs outside, but these are not shown in the diagram), which surrounds it on all sides except at the point *c*. At this point the sheathing becomes thinner, and is folded back inside the tongue, so as to form a duct or tube when the opening at *c* is closed. This seems at first sight a somewhat cumbersome arrangement. "Why not have a simple tube?" the reader will naturally ask. The answer is that a simple tube would be liable to be choked by viscous nectar and grains of pollen, and it is essential that the bee should be able to cleanse it. And here a very beautiful adaptation comes in. When the bee wishes to clean the duct it forces the blood into its tongue; then the whole of the skin forming the duct passes out through the opening at *c*, and the tongue is distended as shown in figure 7. It is, of course, a tube no longer, and admits of being readily polished by the fore-legs.

I ought to add that for figures 5, 6, and 7, and for many of the facts in this paper, I am indebted to Mr. Cheshire's admirable and exhaustive work.

THE GREAT NEBULA IN ORION.

By A. C. RANYARD.

IN a clear night any person with ordinary eyesight may see that the middle star of the sword of Orion is surrounded by a nebulous haze. With an opera glass the phenomenon is easily recognisable, and with a comparatively small telescope what appears to the naked eye to be the central star breaks up into a group of small stars surrounded by a diffused nebulous haze of a faint bottle-green tint.

After the nebula in Andromeda this nebula is the brightest in the northern heavens, but it does not seem to have been known in the pre-telescopic period, and even Galileo failed to discover it though he made a map of this region of Orion containing 400 stars. Cysat of Luzern seems to have been the first to see it in 1618,* and Huyghens gives the earliest drawing of it in his *Systema Saturnium* published in 1659.

Since the days of Huyghens many of our best observers have spent weeks and months in trying to draw this wonderful mass of glowing haze which melts imperceptibly away into the surrounding sky. The difficulties of the task can only be appreciated by those who have attempted it, or by those who have endeavoured to compare and reconcile the drawings made by different observers of a nebula or corona or other hazy object. There are no sharp outlines which can be definitely measured, and the true gradations of brightness are more difficult to represent than would be credited. In comparing such drawings, one has to make allowance for the way in which each observer translates what he sees. Artists have not succeeded well in drawing coronas, though from time to time several artists of distinction have joined in eclipse expeditions with the object of making such drawings. They have generally devoted their energies to reproducing the general effect which has been soft and beautiful, though their pictures have been less truthful delineations of structural detail than those of many observers with less skilful hands.

Professor Holden, the present director of the Lick Observatory, published in 1882 an important monograph on the central parts of the Orion Nebula. It is a profusely illustrated quarto volume and forms Appendix I. of the Washington Observations for 1878. In this monograph Professor Holden discusses all the observations of the nebula he could collect, and he gives some forty woodcuts representing drawings by distinguished observers. On looking over these drawings the casual observer will probably be more struck by their wide discrepancies than by their similarity. While admitting grave discrepancies, Professor Holden thinks that there is sufficient evidence in the drawings to warrant him in stating that certain changes have taken place in the form of the nebula since the date of some of the earlier drawings. I am not convinced of the existence of such a change by an examination of the published drawings. But Professor Holden has had better opportunities of examining and weighing the original evidence, and I do not wish to throw doubt on the existence of such change when I say that most people will prefer to wait until the evidence of change has been registered by the unerring eye of the camera.

The first photograph of the Orion Nebula (and indeed of any nebula in the heavens) was obtained by Dr. Henry Draper at his observatory at Hastings, on the Hudson River, on the night of September 30, 1880. The photograph was taken with a refracting telescope of eleven inches aperture and an exposure of fifty-one minutes. It showed a little more of the nebula than is to be traced on Mr. Isaac Roberts' photograph with six minutes' exposure, which is reproduced at the top right-hand corner of our second plate. On March 11 of the following year another photograph was taken with the same instrument and an exposure of 104 minutes, which showed stars down to the 14·7 magnitude of Pogson's scale and considerably more of the great curved structure marked *a* in the diagram.

In March of the following year Mr. Common photographed this nebula with his 37-inch silver on glass reflector, and obtained a very marked advance on Dr. Draper's results. During this and the following year he took several photographs of the nebula giving exposures up to an hour and a half. The great focal length of his instrument (about 17 feet) exhibited the details of the nebula much more strikingly than they had been shown on Dr. Draper's photographs. The images of stars were sharper, and a still further extent of nebulous area was shown. The structures marked *b*, *e*, and *f*, in the diagram are distinctly traceable. The value of the photographs was immediately recognised, and Mr. Common was awarded the medal of the Astronomical Society for the results he had obtained.

After this date several other observers succeeded in obtaining photographs of the nebula with instruments of various sizes, but the best results yet achieved are due to Mr. Isaac Roberts, who has kindly entrusted me with the custody of his original negatives to make the enlarged photographs from which plates I. and II. have been produced. The instrument with which Mr. Roberts' photographs have been taken is a silver on glass reflector figured by Sir Howard Grubb of Dublin. The reflector has a focal length of 100 inches, and an aperture of 20 inches. The large angular aperture of this instrument, as well as the excellent performance of his driving clock (which was also contrived by Sir Howard Grubb), has enabled Mr. Isaac Roberts to obtain results far surpassing those of Mr. Common, though the scale of his photographs is smaller, owing to the shorter focal length of Mr. Roberts' instrument.

* See a paper by Professor Wolf of Zurich in the 'Astronomische Nachrichten,' vol. xxxviii., col. 109.

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Plate I. is an enlargement of about four diameters from a negative taken by Mr. Roberts on February 4, 1889, with an exposure of 205 minutes. The upper row of pictures on the second plate are enlarged about four diameters from a negative taken on February 16, 1889; on it there are five photographs of the trapezium and central portions of the nebula taken with exposure of 5 seconds, 30 seconds, 1 minute, 3 minutes and 6 minutes respectively. It will be seen that distinct traces of the nebula are shown with some years ago by the late Mr. Newall. Judged by the photographs, this drawing appears to be the most faithful representation of the nebula which any draftsman has succeeded in producing.

Mr. Wesley has assisted me in preparing a diagram, Fig. 1, which will serve as an index map to the more remarkable structures traceable on the photographic enlargements.

(*a*) The great curving structure which we have marked

Fig. 1.—Index Diagram to Structures traceable in the Photographs of the Orion Nebula.

the exposure of only 30 seconds. The four pictures below on Plate II. are enlarged about one and a half diameters from the original negatives, which were taken with exposures of 15 minutes, 30 minutes, 81 minutes and 205 minutes respectively.

The third plate is a photographic copy from a drawing of the Orion nebula made by Mr. Lassell, the original of which is now in the possession of the Royal Society. Professor Holden gives a woodcut of the central parts of this drawing, but the whole drawing, has, I believe, never been published before, though a few autotype copies of it were distributed with the letter *a* on the diagram has a remarkably sharp inner edge with curious notches—which are well shown on some of Mr. Common's photographs as well as on those of Mr. Isaac Roberts. Some of these notches are clearly shown on Mr. Lassell's drawing made a quarter of a century ago at Malta. The structure is narrower and brighter in its lower parts, and it seems to spring from and probably had its origin in the northern (that is lower) parts of the nebula, if not in the region of the trapezium towards which it curves. The outer edge of this structure is much more hazy and ill-defined than the inner edge. The whole

structure is concave towards a line which I propose to call the axis of the nebula, passing through the trapezium in a north-easterly and south-westerly direction.

Structure (*b*) springs apparently from somewhere near the base of structure (*a*). It reminds one forcibly of the tree-like type of solar prominence and of some of the large tree-like structures which are traceable in the photographs of some coronas. But when we remember that no parallax has yet been detected for the stars which are apparently involved in this nebula, the enormous size of the structure as compared with any solar prominence or coronal structure becomes apparent. The stem or trunk of the tree is somewhat smaller or narrower at either end than it is in the middle. It appears to be separated from the cloud-like top of the tree by a narrow rift or channel. The whole structure is slightly concave towards the axis of the nebula. The inner edge of the stem is more sharply defined than the outer edge, and there are some small notches or irregularities on the inner edge, though they are not as striking as those on the inner edge of structure (*a*). This tree-like structure is well shown in Mr. Common's photographs, though it is not quite so hard and definite as on those of Mr. Roberts. It is also clearly depicted in Mr. Lassell's drawing, proving that but little change has taken place in its appearance in the last quarter of a century, so that if there are any analogies between it and the tree-like type of solar prominence which it resembles, the change in this colossal nebulous structure must be very slow as compared with changes in solar prominences. Frequently in a quarter of an hour a solar prominence so changes that its form would not be recognisable, but a quarter of a century has wrought no such changes here.

(*c*) is a smaller tree-like structure which appears to spring from the summit of structure (*a*). It is traceable on some of Mr. Common's photographs, and is very clearly shown on those of Mr. Roberts.

It appears to divide into two branches just below where it reaches the cloud-like summit of structure (*b*); one of the branches (the westerly one) apparently passes through the summit of structure (*b*), and again branches. The whole structure is inclined somewhat towards the axis of the nebula. Mr. Lassell seems to have seen it, and he indicates its presence by a small nebulous extension of the summit of (*b*), and by a cloud-like mass above.

Structure (*e*) also springs from the summit of structure (*a*). It has a narrow stem harder and notched on the inner side, and a large nebulous head. It is traceable on Mr. Common's later photographs, and is shown on Mr. Lassell's drawing, where the stem is given as double, the inner stem being narrow and well-defined.

(*f*) is a gigantic branching structure which either springs from the summit of structure (*e*), or more probably from behind it. It curves over, and is concave towards the axis of the nebula. The inner edge of each of the branches seems to be slightly harder than the outer edge. The whole structure is very similar in appearance to several branching structures, traceable in the photographs of the corona of 1871, which are shown in fig. 2, copied from one of Mr. Wesley's drawings in the eclipse volume of the *Memoirs* of the R.A.S.

(*g*) is a curious forked structure on the other side of the nebula, apparently curving inwards towards the axis of the nebula. Below it are two somewhat similar structures, which though they are not apparent on the enlargements, are distinctly shown on the longest exposed negative of Mr. Roberts. They all curve inwards towards the axis of the nebula.

(*h*) is a tree-like structure, which also bends over towards the axis of the nebula.

(*k*) is a curious canopy of cloud-like loops of nebulous matter, which arches over the summit of the whole nebula. It is shown in Mr. Roberts' negative taken with 81 minutes exposure as well as in that taken with an exposure of 205 minutes. This faint outlying structure was certainly seen by the Earl of Rosse. It is very well shown in his great map of the Orion Nebula published in the *Philosophical Transactions*.

The structure of this nebula seems to be entirely different from that of the Andromeda nebula, or of the nebula in the Pleiades. It seems to exhibit analogies with the structure of the solar corona. In addition to the analogies already pointed out, there seems to be a synclinal tendency of the great curving branches of the nebula similar to the synclinal tendency which is traceable in so many groups of coronal structure. The tendency of the nebular branches to curve together is very recognisable in Mr. Lassell's drawing, as well as in the general outline of brightness traceable upon the photographs.

Fig. 2 is one of the groups of synclinal structure traceable in the photographs of the corona of 1871. The analogy between some of the branching structures there shown and

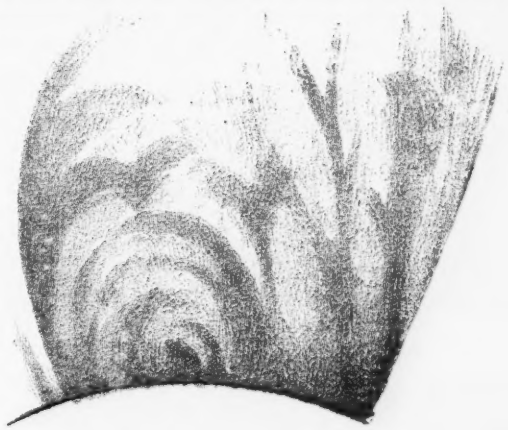

FIG. 2.—GROUP OF SYNCLINAL STRUCTURES FROM CORONA OF 1871.

those in the Orion nebula will be recognised at once. One can hardly rise from an examination of the nebula photographs without the conviction that the structures one has been examining have, like the structures in the corona, had their origin in the lower regions, where they are brightest and narrowest. Some vast explosion seems to have taken place in the region from which the structures appear to spring, and there seems to be in the nebula, as in the corona, some drifting action which tends to curve inwards towards a central axis the great explosive jets.

In this nebula there are no narrow streams of nebulous light stringing together lines of stars, and there are no parallel streaks of nebulous light, as in the Pleiades nebula; nor are there any spiral streams of light arranged symmetrically about a nebulous centre as in the Andromeda nebula or in 81 Messier. A careful study of the strange forms shown in these nebula photographs is evidently one of the first preliminaries to the formation of a theory of the origin of nebulæ, and no amount of speculation is likely to lead to a true theory until such preliminary study has been patiently gone through.

The spectrum of this nebula has long been known to consist of four bright lines on a background of continuous spectrum. Their positions are 500, 495, 486, and 434, of

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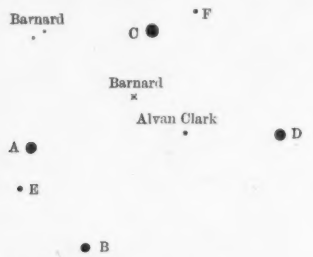
which the green line 500 is by far the brightest. It probably contributes materially towards the greenish appearance of the nebula in the telescope. Many observers have never noticed the greenish tint of the nebula, but when it has once been pointed out to them they never fail to recognise it with a telescope of sufficient aperture. Mr. Lassell used to speak of this nebula as of a pea-green colour; to my eye the colour of bottle-green glass more nearly corresponds to the tint. In June of last year Dr. Copeland communicated to the Astronomical Society a paper on the spectrum of the nebula in Orion, in which he stated that, in addition to the four lines usually seen, he constantly in clear weather saw a fifth line, 587·4 (D_3), the position of which he had pretty exactly determined by thirty-three measurements. He found that the continuous spectrum extended from 568 to 459·5, and that it showed "some indications of resolvability into lines or bands." In another paper, read in January of this year by Mr. A. Taylor, four additional faint lines are said to have been seen, viz., 559, 520, 470, and 447.

As all the great structures of the Orion nebula curve towards the region of the trapezium, one is inclined to look to that region for stars which may by their collision have given rise to a tremendous explosion. If the stars of the trapezium are fragments of the colliding masses, they show no motion which has as yet been detected, and the larger stars of the group exhibit but slight variability of brightness if any.



Stars of the Trapezium.

The above diagram is from a drawing by Mr. Burnham, who is of opinion that even the smaller stars are not variable. Other observers, however, differ from him as to this, though none have at present produced evidence tending to show that there is any relative motion amongst the members of this interesting little group, which has, perhaps, been more closely studied by the possessors of large telescopes than any other equal region in the heavens. The Orion nebula lies in the midst of a region rich in nebulous stars, nebulous patches of light, and variable stars—extending certainly as far as the stars of Orion's belt. In the nebula to the north of the great nebula (shown near the bottom of plate 1) there seems to be a certain degree of symmetry and analogy of structure to the great nebulous mass around the trapezium, though the analogy is not at first sight very obvious. The nebulous light seems to curve away on either side from a central star, and there is to the north of it a cloud-like canopy, with a hard inner edge turned towards the chief mass of the nebula, as in the case of the great nebula, where, however, the cloudy canopy is fainter.

What is probably the first submarine telephone cable has been recently laid between Buenos Ayres and Montevideo across the wide estuary of the Rio de la Plata. It is of special form, allowing both telegraphy and telephony simultaneously on the same wires.

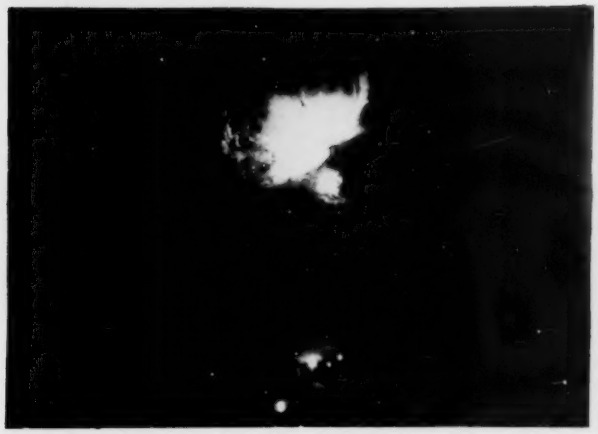
Notes.

Mr. Burnham has found with the 36 in. of the Lick Observatory that α Ursæ Majoris is a close double star, but a very unequal and difficult one. The mean of his two measures gives for the position and distance of the small star 326°·5, 0″·89. The companion is of the 11th magnitude in the photometric scale, or the 16th in Herschel's and Smyth's, and is not a very easy object, except on very good nights, in the great telescope. This discovery is interesting, as *Dubhe* has a proper motion of 14½ seconds of arc in a century, in which a small distant 7th magnitude star of a violet colour shares. Huggins found that the large star is approaching us with a velocity of from 47 to 62 miles a second in 1872. The Greenwich observations with the "half-prism" spectroscope in 1880 and 1885 give a very small approach of 4 miles a second, those in 1886 58½ miles a second, while Mr. Seabroke's rather discordant results at Rugby in 1885 show a rate of approach of 16 miles a second.

A FORGOTTEN IMPROVER OF THE STEAM-ENGINE.

Notes and Queries gives a reprint of a "case," which was printed for distribution in the House of Commons in the year 1726. It is that of one Walter Lynn, M.B., "in relation to divers Undertakings of his, particularly for the Improvement of an Engine [*i.e.* Savery's] to raise Water by Fire. . . . . offer'd to the Parliament in 1726." Lynn gives no specific description of the engine's mechanism, though he enlarges upon its great economy of fuel, and mentions the "one" engine in Westminster, and that like to be a "Grievance to the poor and almost all sorts." He asks for some reward, and for a support and maintenance, as having showed myself absolutely the best geometrician in the nation—*testibus* Sir Isaac Newton and others. The statement concludes with a certificate signed by Sir Christopher Wren. Christopher Wren (the son), and Dr. Taylor, bearing testimony to the value and practical advantages of Lynn's invention. The reference to the Westminster engine is noteworthy. We take it to be what was popularly known as "the York Buildings Dragon," by the Adelphi, which Savery constructed, after 1712, on the model of one which he had made in that year for a Mr. Balle, at Campden House, Kensington. That famous machine is cited by such writers as Dr. John Allen, in his "Specimina Ichnographica," 1730; Switzer, in his "Hydrostatics and Hydraulics" of 1729; Dr. Degaguliers, in his "Experimental Philosophy," and Bradley, in his "Ten Practical Discourses," 1727. A description of its working will be found in the "Foreigner's Guide to London," 1720. The York Buildings Waterworks Company was originally incorporated in the year 1690. This particular "fire engine" ceased to be used in 1731, having given great offence to the neighbourhood by the volumes of sea-coal smoke that escaped from its furnace. But the wooden water-tower, rising about 65 feet, long survived, and forms a conspicuous feature in certain views of York Buildings of about 100 years ago.

From a large number of experiments made upon a variety of pigments, Captain Abney has found that "Mineral colours are far more stable than vegetable colours; amongst those colours which have remained unaltered, or have very slightly changed after exposure to light of extreme severity, a good gamut is available to the water-colour artist. The presence of moisture and oxygen are in most cases essential for a change to be effected even in the vegetable colours. The exclusion of moisture and oxygen, particularly when the latter takes the form of ozone, would



PHOTOPHANE, 846 OLD KENT ROAD, LONDON.

THE GREAT NEBULA IN ORION.

The Five Pictures in the top line are enlarged about four diameters from Negatives taken in the exposures of 5secs., 30secs., 1min., 3min., and 6min. The Four lower Pictures are enlarged about one and a half diameters from Negatives taken with exposures of 15min., 30min., 81min., and 3½ hours.

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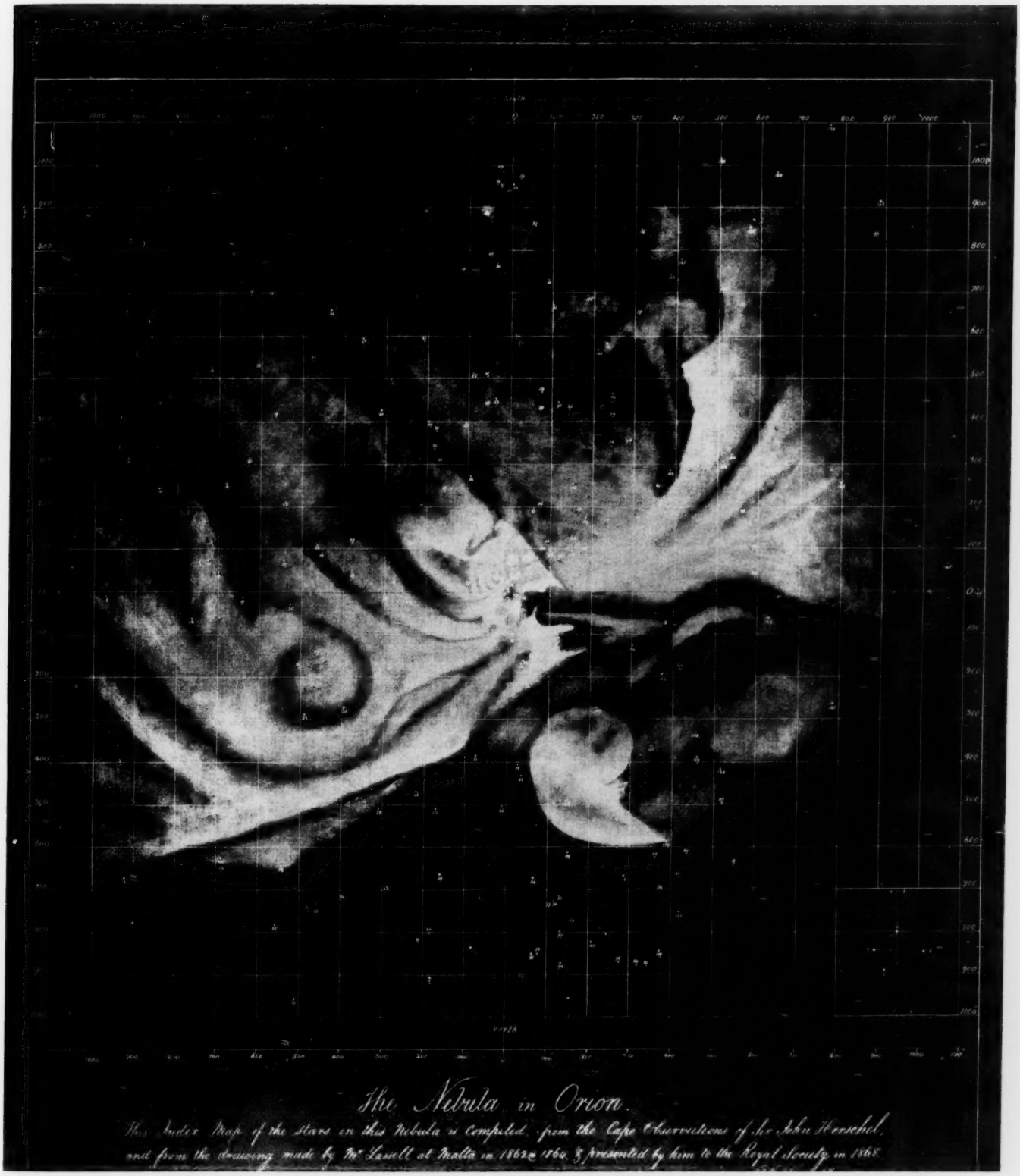
The Nebula in Orion.
This Index Map of the Stars in this Nebula is Compiled from the Cape Observations of Sir John Herschel,
and from the drawing made by Mr Lassell at Malta in 1862 & 1864 & presented by him to the Royal Society in 1865.

MR. LASSELL'S DRAWING OF THE ORION NEBULA.

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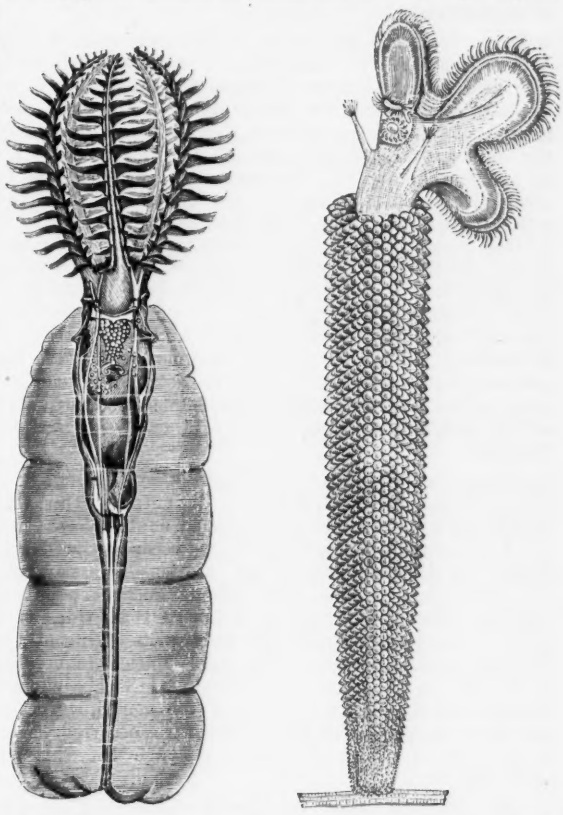
give a much longer life even to these than they enjoy when freely exposed to the atmosphere of a room. It has also been proved that every pigment is practically permanent when exposed to light *in vacuo*, a fact which indicates the direction in which experiments should be made for the preservation of water-colour drawings. The effect of light on a mixture of colours which have no direct chemical action on one another is that the unstable colour disappears and leaves the stable colour unaltered appreciably. The experiments further show that the rays which produce by far the greatest change in a pigment are the blue and violet components of white light, and that these for equal illumination predominate in light from the sky, while they are less in sunlight and diffused cloud light, and are present in comparatively small proportion in the artificial light usually employed in lighting a room or gallery.

AT the meeting of the Meteorological Society, held at the Institution of Civil Engineers on April 17, a paper was read "On the Deaths caused by Lightning in England and Wales from 1852 to 1880, as recorded in the Returns of the Registrar-General," by Inspector-General R. Lawson, LL.D. The total number of deaths from lightning during the twenty-nine years amounted to 546, of which 442 were of males, and 104 of females. In consequence of their greater exposure, the inhabitants of rural districts suffer more from lightning than those of towns. It appears also that vicinity to the west and south coasts reduces the chances of injury by lightning, and that distance from the coast and highland seems to increase them.

Notices of Books.

Practical Microscopy. By GEORGE E. DAVIS, F.R.M.S., F.I.C., &c., &c. New and Revised Edition. (London: W. H. Allen & Co.)—This is a complete revision of the well-known "Practical Microscopy," with much additional matter. The enormous annual increase in the number of microscopes sold by the dealers, and the consequent increased number of workers with the instrument, prove the timeliness of the new edition. There was also the additional fact that no such valuable guide for the beginner was to be had. Other books, indeed, there were, but these were either too juvenile on the one hand or too advanced on the other. Mr. Davis's work is one which a student already advanced in scientific methods will read with pleasure and profit at the time when he is commencing the use of the microscope, and also one which will serve him for some time after he has become acquainted with its manipulation. It is no slipshod catchpenny for idle "half-hours," but a book conveying in twenty chapters, written in an easy and graceful style, really genuine teaching. The author's ideal of the use of the microscope is a lofty one, and this ideal will be infused into the readers of his book. "The microscope," he says, "is a civilising instrument." There is no doubt about this. Many a person has been turned by its influence from habits of haphazard guessing about things in general, to habits of close observation of things in particular. For persons of too much assurance, a course of exercises in the use of the microscope is calculated to prove very beneficial. For persons who pride themselves in an unusual amount of common sense, and think they can do something better than *learn*, the microscope is an enlightener. Its uses are thus moral as well as educational. Purchased at first, perhaps, as a mere curiosity, it has often led its owner into realms of thought and observation not before conceived to exist. He must be a veritable dull-head who never passes the first stage, or whose appetite is not whetted by the first few glances into the otherwise unseen world which it affords. But if the owner appreciate the undoubtedly wonderful revelation vouchsafed him, he will inevitably seek out some systematic work to aid him in putting to the greatest degree of utility his new instrument—we had almost said his new *sense*, for such it often seems to be; and happy he will be if the work now under notice fall to his hand. The amount of heartburning over the search for a really genuine and useful treatise on the microscope, which shall intelligently explain both the instrument itself and the varied classes of objects to the study of which it may be applied, and be at the same time comparatively inexpensive, can perhaps hardly be estimated by those not familiar with very much of it. This is pre-eminently *the* book for one who has a microscope and does not know what to do with it. To such as are engaged in definite advanced lines of study requiring the



application of the microscope, we should not recommend this book. The aids which these students require are usually much more costly. But the number of owners of microscopes who are not strictly to be regarded as scientific students multiplies as rapidly as the number of those who are, and to these we cordially recommend Mr. Davis's manual. Nor is it to be inferred that working students of science will not find the book useful. On the other hand, there is perhaps no other book which so well serves the purposes they will require until considerable advance has been made. An introductory chapter deals with simple magnifiers, such as most microscopists usually carry in their pocket, and the images cast by these are explained. Then the compound microscope is described in detail, with all its most useful "accessories." The patterns of all the principal makers, in-

tended for their varied purposes, are described and figured. The value of all this to any one about to obtain a microscope need not be pointed out. We notice that the author wisely recommends binoculars when their use is available. The chapter on the human eye, which might have been considered dispensable, only shows with what thoroughness our author has done his work. It is, moreover, very ably condensed. The leading facts of the aperture question are explained mainly on the lines of Mr. Blackburn's paper, read before the Manchester Microscopical Society—a paper which is classical. If the book meet with any fault-finders, the complaint will probably be that the fault-finder's particular hobby or study has not met with fuller treatment. The impracticability, however, of pleasing everyone in this case is apparent. There are 436 octavo pages, and 310 illustrations. Our readers will judge of the excellence of the latter by an examination of two which we reproduce, and which represent living objects of great beauty familiar to all microscopists.

Thirty Thousand Years of the Earth's Past History. By Major-General A. W. Drayson. (Chapman & Hall.)—In this little brochure General Drayson attempts to prove, by means of a mysterious "second rotation" of the earth, that the pole of the equator describes a circle round the pole of the ecliptic in 31,682 years, and not in 25,800; and if this is admitted he thinks "the reader . . . will be able to accomplish by calculation, in a few hours, more than the astronomers of the whole world have been able to arrive at by their perpetual observations during the past two hundred years." He also apparently thinks that precession is described by astronomers as a motion of the earth's axis on one of its poles, the other remaining fixed. On what observed facts General Drayson's paradox is founded we have been unable to discover. It is clear that he does not understand the generally received theory by which the action of the sun and moon on the earth's equatorial protuberance produces a shift of the earth's axis. This book would have delighted De Morgan.

Glimpses of Feverland. By Archer P. Crouch, B.A. (Sampson Low, Marston, Searle & Rivington.)—This very interesting account of life and customs on the West Coast of Africa forms the conclusion to "On a Surf-bound Coast," by the same author, and is ostensibly a narrative of cable-laying between St. Thomé and St. Paul de Loanda. But it is in reality far more than this, as it gives a vivid and picturesque description of life at Old Calabar, and at the various Portuguese settlements on the coast, cast in the form of a diary. Curiously enough, though the author gives the days of the month, we get no hint as to the year in which his adventures occurred. From a notice of an eclipse of the sun, on page 183, this was evidently 1886.

The Student's Atlas, in Twelve Circular Maps. By R. A. Proctor. (Longmans, Green & Co.)—In this capital little work, which should be in the hands of every schoolmaster, the distortion shown in ordinary atlases is avoided, and the student can see at a glance the true relation of the various countries of the globe to one another. Maps I. and XII., for instance, show the Arctic and Antarctic regions, and their positions with regard to Europe, Asia, and North America in the one case, and the extreme south of South America and the islands south of New Zealand in the other, in a manner in which they "have never been presented before." This is one of the many new ideas available for teaching purposes contrived by Mr. Proctor, who had a special gift for devising new methods of projecting the sphere for charting purposes. The ocean currents are carefully laid down, perhaps in a shade too dark a tint, while on the two index maps the mean solar time on the corresponding meridians, when it is mean noon at Greenwich, is indicated.

The Naturalist in Siluria. By Captain Mayne Reid. (Swan Sonnenschein & Co.)—This is not, as might have been inferred from its title, a geological treatise, but simply a collection of short magazine and newspaper articles on the fauna of a small portion of the Wye Valley. Captain Mayne Reid shows a strong tendency to repeat himself, while some of his statements, *e.g.* that "amid the lush vegetation of the moist Wyeside woods sings and stings the mosquito (*Culex pipiens*)" are singularly inaccurate. *Culex mosquito*, which has very rarely been observed in England, is the mosquito of the tropics, *Culex pipiens* is the common gnat. On page 8 the honey buzzard is called *Falco apivorous* for *Falco apivorus*, and the barn owl *Strix flammeu*, instead of *Strix flammea*.—H. S.

Our Earth and its Story. Vol. II. Edited by Robert Brown, Ph.D., F.R.S. (Cassell & Co.)—In the first volume of this excellent popular work Dr. Brown treated on volcanoes, the action of rivers, glaciers, &c.; in the present one he discusses the readings from the rocks. As a popular treatise on geology and palæontology the present work will be valued, and the illustrations are copious and good, though several of them have done yeoman's service before; figure 175, a funeral feast in the palæolithic age, is a very old friend. The work is one to be recommended for boys.—H. S.

Letters.

[The Editor does not hold himself responsible for the opinions or statements of correspondents.]

SELF-COMPENSATING PENDULUM.

To the Editor of Knowledge.

Sir,—My experience with transit clocks in which the pendulum is hung by a spring is, that when it is necessary to stop the clock for any purpose, you can never depend on the spring bending at the same point when the pendulum is again set in motion, even if the stoppage be only for a few seconds. Often I have lengthened a pendulum and then found it gain, or shortened it only to make it lose more. One can never be certain what the pendulum will do after being stopped. It is in consequence of this experience (extending over a great many years and with a large number of clocks) that I do not expect Mr. Inwards will find his method of compensation to answer very well.—Yours faithfully, G. L. Tupman.

SOLAR SPOTS *AND* VARIABLE STARS.

To the Editor of Knowledge.

Sir,—In my article in your January number I suggested an analogy between sun-spots and the phenomena presented by variable stars of the Mira type. I had previously made the same suggestion, and very likely it did not originate with me, though I do not recollect having seen it in print elsewhere. I wish now to call attention to the discovery of bright lines in the spectra of several variable stars of this type since the appearance of my article, while the latest text-book on the subject (Professor Young's "General Astronomy") says of the spectrum of a solar spot, "This is like the general solar spectrum, except that certain lines are much widened, while certain others are thinned, and sometimes the lines of hydrogen become bright" (p. 200). Besides the hydrogen lines, however, the D lines were

noticed as reversed in the Greenwich observations on the great sun-spot of November 1882 ("Monthly Notices" for January 1883), while D_1 and D_2 were, besides being reversed, "extravagantly broadened"—the very feature, if my memory does not deceive me, which is most conspicuous in the Harvard photograph of the spectrum of Mira. A broadening of many other solar lines was also observed. I think, therefore, that I am justified in concluding that, if the greater part of the sun's surface was covered with well-developed spots, the solar spectrum would not be dissimilar to that of Mira, while its light would be considerably diminished.—Truly yours,

April 20, 1889. W. H. S. MONCK.

MAGIC SQUARES.

To the Editor of KNOWLEDGE.

SIR,—As attention has been drawn to the subject of magic squares in your interesting and valuable magazine, I beg to enclose a few remarks on the subject which you may think worthy of insertion. First as to the mode of formation. We may roughly divide these squares into two classes, the odd and the even. The odd squares are the easiest to construct and may be mechanically reproduced as follows:—We have to arrange the (n^2) natural numbers in the form of a determinant the sum of whose rows, columns, and diagonals shall equal $\frac{1}{2}n\ (n^2+1)$ when n is greater than 2. Let $n=5$ then, our magic square will be

17	24	1	8	15
23	5	7	14	16
4	6	13	20	22
10	12	19	21	3
11	18	25	2	9

Begin in the top central square, and follow the numbers 1, 2, 3, &c., proceeding diagonally. If any square be occupied, place underneath and go on as before. When the edge of the square is reached go to the extreme square of the neighbouring row or column. One diagonal always forms a sequence.

Other modes of formation are:

(2)

1	20	16	23	5
15	7	12	9	22
24	18	13	8	2
4	17	14	19	11
21	3	10	6	25

(3)

11	18	25	2	9
10	12	19	21	3
4	6	13	20	22
23	5	7	14	16
17	24	1	8	15

In (2) each diagonal is arranged as an arithmetical progression.

The even squares are not so easy to construct as the odd ones, but if we make the principal diagonals consist of two arithmetical progressions the remaining squares are easily filled up from the remaining numbers, since they work in pairs whose sum equals n^2+1, thus $15+2=14+3=12+5=9+8$.

1	15	14	4
12	6	7	9
8	10	11	5
13	3	2	16

Here we have ($n=4$).

$$1+(n+2)+(2n+3)+(n^2 \text{ or } 3n+4)$$
$$=n+(2n-1)+(3n-2)+(4n-3)=\tfrac{1}{2}n(n^2+1)$$

A few of the properties of these really curious squares are as follows:

Take the numbers in the top and bottom row and square them, and we have

$$1^2+15^2+14^2+4^2=13^2+3^2+2^2+16^2;$$

the second and third rows, and we get

$$12^2+6^2+7^2+9^2=8^2+10^2+11^2+5^2.$$

Similarly for the side rows,

$$1^2+12^2+8^2+13^2=4^2+9^2+5^2+16^2$$
$$15^2+6^2+10^2+3^2=2^2+11^2+7^2+14^2$$
$$1^2+6^2+11^2+16^2=2^2+4^2+13^2+15^2$$
$$1^2+8^2+12^2+13^2=2^2+7^2+10^2+15^2$$
$$14^2+4^2+7^2+9^2=8^2+10^2+13^2+3^2$$
$$1^2+15^2+12^2+6^2=11^2+2^2+5^2+16^2$$
$$4^2+7^2+10^2+13^2=3^2+9^2+10^2+12^2, \text{ \&c. \&c.}$$

(2) Since the roots are equal, we can raise or depress these equations *ad libitum*. Thus

$$2^2+16^2+15^2+5^2=14^2+4^2+3^2+17^2$$
$$11^2+5^2+6^2+8^2=7^2+9^2+10^2+4^2, \text{ \&c. \&c.}$$

(3) This square (since $n=4$, which is greater than 3) will give us equations also for cubes, thus *e.g.*:—

$$1^3+6^3+11^3+16^3=2^3+4^3+13^3+15^3$$
$$2^3+8^3+9^3+15^3=3^3+5^3+12^3+14^3$$
$$3^3+7^3+10^3+14^3=4^3+5^3+12^3+13^3, \text{ \&c. \&c.}$$

It is unnecessary to remark that the indices may be depressed (and in higher squares raised) without falsifying the equations. An example of this unique kind of equation I have worked out as follows in a paper read before the Mathematical Society, and which included the theory of magic squares:—

$$2^5+3^5+13^5+15^5+25^5+26^5$$
$$=1^5+5^5+10^5+18^5+23^5+27^5$$

Here the indices may be 1, 2, 3, 4, or 5 indifferently, and the coefficients of course raised at pleasure.

I will conclude this brief account, which might easily have been lengthened, by giving the figures for an eight square, which is the largest *even* one I have yet formed, though there is little doubt that squares of any magnitude, odd or even, may be arranged.

1	48	44	60	61	21	17	8
56	10	39	54	11	26	15	49
47	35	19	59	6	22	30	42
40	52	2	28	29	63	13	33
32	14	62	36	37	3	51	25
18	31	43	7	58	46	34	23
9	50	27	12	53	38	55	16
57	20	24	4	5	41	45	64

I am, Sir, yours truly,

Great Crosby. ROBT. W. D. CHRISTIE.

THE CRIES OF FROGS.

To the Editor of KNOWLEDGE.

SIR,—In the interesting article on "The Common Adder," by Mr. A. J. Field, in the February number of KNOWLEDGE, when describing the cut at the end of the article, he says, "Such cries are only heard from frogs when in the power of snakes." Allow me to record the following fact: In the

month of September 1865, at Cawnpore, I was awakened at about twelve midnight by hearing a most piercing shriek. As it was again repeated I got up to find out the cause. The scream came from a frog (not a toad) which had been seized by the hind leg by a musk rat, and who was trying to drag the frog under the bottom of the door of my bedroom into the verandah outside; the door was not quite closed; the scream was repeated several times. I got my stick, killed the rat, and released the poor frog. It, however, appeared to be more frightened than hurt. It is not an unusual thing for frogs to enter houses in India, especially during the rains. I for one do not turn them out, as they kill beetles and other insects, and do no harm. Mr. Field does not describe the cry he heard, but what I heard was like a shrill scream of a young child; but in the stillness of the night it sounded to me more piercing.

Twenty-seven Years of India.

Harda, Central Provinces, India.

To the Editor of Knowledge.

Sir,—The communication from "Twenty-seven Years in India" is interesting, but it is a pity the writer has not stated the species of frog whose cries resembled those of a child. Presumably it was the Indian bull-frog (*Rana tigrina*), which is common in all parts of Hindostan, and its great vocal power I am well aware of, though I have not detected the slightest resemblance between its cry and the scream of a child. I once had two or three dozens of these frogs, together with some other species, that had been sent to me for identification.

I injudiciously let them share my bedroom. The result was that when just dozing off into my first sleep, a noisy chorus of song was commenced by the frogs, and, for fear of their disturbing the other members of the household, I was obliged to get up and deposit the frogs in the garden.

The true musk-rat (*Fiber tibethicus*) is a native of America. The animal referred to by "Twenty-seven Years in India" is, I imagine, the Indian shrew (*Sorex indicus*), which, owing to its musky odour, has received the name of "musk-rat," and thereby confused many readers. This Indian shrew is well known for the sharp, shrill cries it utters, more particularly during the night-time. Is "Twenty-seven Years in India" quite certain that the cry proceeded from the frog and not from the shrew? In common with the frog, the shrew feeds upon cockroaches and other insects. An interesting question arises—What was the shrew going to do with the frog? As to the cry uttered by frogs when in the power of snakes, so far as my experience goes, it is a low, hoarse, and lamentable note, and cannot be compared to that of any other cry. It is totally unlike the scream of a child.

A. J. Field.

A METHOD OF ASCERTAINING THE FUNDAMENTAL COLOURS.

To the Editor of Knowledge.

Sir,—Hitherto the advocates of Young's theory of colour appear to have relied mainly upon mixture experiments for determining the fundamental colours, and, owing to the ambiguous character of the evidence so obtained, have not all arrived at the same conclusion. Young selected red, green, and violet as the fundamental sensations, suggesting that blue may be accounted for as a combination of the green and violet sensations, since it can be produced by mixing green and violet light (not pigments). Helmholtz, after extensive investigation of the subject, endorsed Young's theory and his selection of fundamental colours; but Clerk Maxwell, also an advocate of Young's theory, was led by independent investigation to the conclusion that blue is fundamental, and violet a combination of the blue and red sensations. Considering the positions of the colours in the spectrum, violet seems a more probable fundamental than blue; for, as Professor Rood has pointed out, "if the end and middle colours of the spectrum (red, green, and violet) be not selected, then one of the three must have two maxima, one in the red and the other in the violet, which is a more complicated but not an impossible supposition" ("Modern Chromatics, Int. Sc. Series," p. 115). This may account for the more general acceptance of violet, notwithstanding that the evidence derived from colour mixtures is not specially favourable to it. After the sentence just quoted, Professor Rood writes:—"The only known method of deciding this point is by the investigation of those persons who are colour blind." Experiment, however, has convinced me that the question can be definitely settled by a simpler and more certain method, the neglect of which is the more surprising since it is but a particular application of facts which have been long recognised and often described (*v.* "Modern Chromatics," pp. 236–9). The green, ghostly image seen on a yellow surface, after gazing steadily for a short time at a small piece of red paper, may be cited as an example of the facts alluded to. It is a well-established fact that long gazing at any bright colour renders the eye temporarily less sensitive to that colour; therefore, when the eye has been thus rendered insensitive to one of the components of a colour which is a compound sensation, that colour may be expected to assume a different tint inclining towards that of its other component. The green "ghost" seen on a yellow ground after gazing at a fragment of red paper can thus be satisfactorily accounted for, yellow, according to Young's theory, being compounded of green and red. Experiments of this kind, however, cannot be relied upon for distinguishing between compound and fundamental colours, for three reasons—(1) on account of the complex nature of pigment colours or dyes; (2) because all coloured materials, such as paper, ribbon, &c., reflect a certain amount of white light from their surface, a circumstance tending to give the ghost a colour complementary to that of the paper gazed at; and (3) owing to the disturbing effects of contrast between the colour of the "ghost" and that of the ground upon which it is seen. The unreliability of such experiments is well shown by these two facts, viz., that a blue "ghost" is seen on a violet ground after gazing at a small piece of red paper, and a green "ghost" on a blue ground after gazing at violet; whereas it is clearly impossible that both violet and blue can be compound sensations, as the experiments seem to indicate. Very different results are obtained with the simple colours of the spectrum, if the requisite insensitiveness of the eye be produced by gazing for about a minute, preferably at the sky, through richly stained glass. It is best to affect one eye only, retaining the other in its normal condition for the sake of comparison in case the changes of tint should not be at once obvious. Proceeding thus, and using a direct-vision spectroscope, I have observed the following results, all of which have been confirmed by friends who were not previously told what results to expect. After exposure of the eye to red light, the yellow of the spectrum appears to have changed to a very green yellow, and even the orange appears greenish. Exposure of the eye to green light causes the yellow to become orange, and the orange nearly red. These changes, which of course are only transient, confirm Young's hypothesis that yellow and orange are compounded of green and red. Still more striking changes can be effected in the colours lying

between green and blue. Ordinarily the Fraunhofer line F very nearly marks the position in the spectrum where blue merges into green; but after the eye has been sufficiently exposed to green light, the blue will be seen extending much farther towards the red end of the spectrum to a position midway between lines E and F. It may here be mentioned that a similar effect in less degree is produced by exposing the eye to red light devoid of green or yellow rays. This fact, which is too interesting to be left unnoticed, is quite consistent with Young's theory; but space will not permit of more than a bare mention of it. It should likewise be observed that exposure of the eye to blue light also slightly lessens the sensitiveness of the eye to green, thus causing the yellow of the spectrum to assume a slight orange tint. It also causes extension of the green towards the violet end of the spectrum owing to the temporary conversion of green-blues into blue-greens. These remarkable results lead us to expect that blue, if a compound sensation, will show corresponding changes of tint; but by no length of exposure of the eye to green or violet light have I succeeded in causing the central portion of the spectral blue to assume a violet or green tint respectively. It seems impossible to account for this except on the assumption that blue is a fundamental colour; and for similar reasons we may also infer that red and green are fundamental colours. Violet, on the contrary, exhibits marked changes of tint, appearing bluer after exposure of the eye to red light, and more red when the eye has been exposed to blue light, which results at once suggest that it is a compound of blue and red. The changes in violet can be more conveniently observed with a common prism; the inconvenience of rapidly shifting the spectroscope from one eye to the other, in making the necessary comparisons, being thereby avoided. If the prism be held horizontally with its refracting angle upwards, a band of violet will be seen fringing the top of a window frame backed by the sky. If neither spectroscope nor prism is available, an examination of any rich blue and violet material will serve to indicate that blue is a more probable fundamental than violet; but the use of the stained glasses for fatiguing the eye cannot well be dispensed with. The blue resulting from mixtures of green and violet is probably the excess of colour left over after the red of the violet, and a portion only of its blue, have combined with the green to form white or grey. The pale and somewhat slaty appearance of the blues so obtained agrees well with this explanation; and mixture experiments show that violet, if compound, must contain blue largely in excess of red. F. W. REYNOLDS.

March 10.

[The terms "fundamental colours" and "primary colours" imply something which does not exist. It would be better always to speak of "primary colour sensations," for no one colour can be said to be more "primary" than another, except that some correspond to the sensation produced by a mixture of wave-lengths and some to the sensation produced by a single wave-length. There are evidently in the retina of the human eye three classes of organs, which correspond to nerves which communicate to the brain different colour-sensations. The question is, what colours do these three sets of sensations correspond to? It is well known that there is an outer elliptical zone in our retinas where we cannot perceive red light, and that there are certain substances—such, for example, as santonine—which make nearly all persons incapable for the time of perceiving violet colours. The method proposed by Mr. Reynolds is not altogether satisfactory, as the coloured glasses he uses correspond to a considerable range of wave-lengths in the spectrum, and people are not at all times equally sensitive to the paralysis of the colour-sense brought on by staring steadfastly at a bright light. The method of tiring the nerves has been tried before, and has frequently been described. But, as far as I am aware, Mr. Reynolds's method of comparing the sensations of the tired eye with the spectrum-sensations is new.—EDITOR.]

THE FIRST INVENTOR OF THE ACHROMATIC TELESCOPE.

BY W. T. LYNN, B.A., F.R.A.S., *formerly of the Royal Observatory, Greenwich.*

ALTHOUGH Dollond first brought achromatic telescopes into general use after taking out a patent for their manufacture, and there is no reason to doubt that he independently discovered the method of combining two lenses so as to achromatise one another, it is now well known that achromatic object-glasses had been made a quarter of a century before by Chester Moor Hall. The editor of this journal has brought to light many facts respecting Moor Hall which were previously not generally known, anything relating to him must be of interest to astronomers, and some additional information may be found in what follows.

Before Mr. Ranyard published his researches so little was known about Moor Hall that the most ludicrous mistakes respecting his name are to be found in many foreign books. Thus Montucla ("Histoire des Mathématiques," vol. iii., p. 448, *note*) calls him Chestermonhall, and in so recent and excellent a work as Wolf's "Geschichte der Astronomie" we read (p. 585), "Schon um 1733 soll es auch einem 'Esquire, of More Hall, in Essex,' namens Chester, wirklich gelungen sein einen kleinen Achromaten zu construiren." I suppose no one wishes now to endorse the view expressed by Lalande to Montucla, that Dollond was assisted in his discovery by any knowledge he may have obtained of Hall's success in making achromatic glasses twenty-five years before he took out his own patent in 1758. But surely we may have some doubt about the applicability of the expression in Lord Mansfield's famous dictum in the Dollond *v.* Champness patent trial respecting locking up a useful invention in an escritoire. It does not appear that Hall took any steps to make his invention generally known; but neither does it appear that he had any desire that it should be kept a secret. Being a man of large independent means, he had no motive for protecting his rights; but the evidence given in the action brought by Peter Dollond shows that Hall's workmen were well acquainted with the nature of their instructions.

The family of Chester (with which that of Hall was connected) seem at one time to have had property at Harlow (near which there is a Moore Hall) in Essex; for it is mentioned in Morant's "History and Antiquities of Essex" that a Robert Chester had defrayed the expense of putting up a curious rail about the font of Harlow Church. But he was also in possession of the estate of Stilemans, in the parish of Wickford, in the southern part of the county, on the river Crouch. This estate passed into the hands of his sister's son, John Moore, whose grandson, Chester Moore, left it to his sister's son, John Hall, the grandfather of Chester Moor Hall, counsellor-at-law, and bencher of the Inner Temple, the first inventor of the achromatic telescope. It appears from Morant that, besides the estate of Stilemans at Wickford, which he inherited from his paternal ancestors, he came into possession through his mother of the estate of New-House, in Sutton,

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near Rochford, and it was there that he died, as is stated on his tombstone in Sutton Church (see Mr. Ranyard's letter, and the communication from Mr. King annexed to it, in the "Astronomical Register" for August 1881), on March 17, 1771, at the age of sixty-seven, having been born at Leigh (formerly spelt Lee, and still pronounced like the Kentish village of that name) about the end of the year 1703.

It is somewhat singular that in the paper which Peter Dollond communicated through Maskelyne to the Royal Society in 1789 (which was read but not published* by them, in consequence of which he afterwards published it independently), defending his father's claim to be the first discoverer of the principle of the achromatic combination of lenses, no reference whatever is made to Hall. I am also surprised to find no mention of him in the generally excellent "History of the Telescope" ("Geschichte des Fernrohrs"), which was published by Dr. H. Servus at Berlin in 1886.

THE FACE OF THE SKY FOR MAY.

By Herbert Sadler, F.R.A.S.

THE sun's disc still remains almost absolutely free from spots. Minima of the Algol type variable, δ Libræ (*cf.* "The Face of the Sky" for April), which can be conveniently observed, occur at 11h. 50m. P.M. on the 6th, at 11h. 25m. P.M. on the 13th, at 10h. 59m. P.M. on the 20th, and at 10h. 33m. on the evening of the 27th. Mercury is in a very favourable position for observation during the last half of the month. He is in perihelion at 9h. P.M. on the 1st, but on the 14th he sets at 9h. 43m. P.M., or just two hours after the sun, and on the 21st at 10h. 4m., two and a quarter hours after sunset. On the 24th he attains his greatest eastern elongation ($22\frac{3}{4}°$), setting on that evening two hours and ten minutes after the sun, and on the 28th he sets two hours after the sun. Having considerable northern declination, he may be easily picked up with the naked eye in the N.W. during the last fortnight in May. He passes from Aries to Taurus on the 3rd, and enters Gemini on the 26th, but does not make any close approach to a naked-eye star. At about 8h. P.M. on the evening of the 30th he will be less than 6′ *n.p.* the $6\frac{3}{4}$ magnitude star 5 Geminorum. Venus, as we mentioned in the April number of Knowledge, comes into inferior conjunction with the sun at 2h. A.M. on May 1, and is a morning star throughout the month. On the 14th she rises just an hour before the sun, with a northern declination of $14\frac{1}{2}°$, and an apparent diameter of 53″. She then presents the appearance of the moon a little less than two days before she is new. During the latter half of the month Venus becomes a brilliant object in the morning sky, rising on the 31st one hour and twenty-three minutes before the sun, having a northern declination of $11\frac{1}{2}°$, and an apparent diameter of 42″. She describes a short retrograde path in Aries during the first three weeks, being stationary at noon on the 20th, but she does not approach any star visible to the naked eye at all closely. Mars is, for the purposes of the observer, invisible throughout the month. Jupiter rises on the 1st at one minute before midnight, and on the 31st at 9h. 53m. P.M., but he is as badly placed for the student as he well can be, with a southern declination increasing from 22° 56′ on the 1st to 23° 5′ on the 31st. His equatorial diameter increases from 42·3″ on the 1st to 45·4″ on the 31st. The following phenomena of the satellites occur between the time of the planet's being 8° above the horizon and midnight G.M.T. on the days named. At 11h. 39m. P.M. on the 24th a reappearance from occultation of the second satellite, and at 11h. 37m. P.M. on the 29th an egress of the shadow of the first satellite. Jupiter describes a short retrograde path in Sagittarius, but does not approach any naked-eye star. On the 18th, at 3h. 7m. A.M., or about an hour before sunrise, he will be 15′ south of the moon. Saturn is still in a favourable position for observation, though he is receding from us, being in quadrature on the 4th, his apparent diameter decreasing from 17·2″ on the 1st to 16·4″ at the end of the month. He souths at 6h. 27m. P.M. on the 1st, and sets at 13 minutes after midnight on the last day of the month. At 7h. 5m. P.M. on the 8th, Titan will be 50″ north of the planet, at midnight on the 16th, the same satellite will be 51″ south of Saturn, and on the evening of the 24th Iapetus will be south of Saturn at 30″ distance, and Titan north at 47″. The student should carefully examine Saturn on the evening of the 18th, as it is quite possible that an occultation of a 9th magnitude star may occur. Such phenomena are very rare, though Gottfried Kirch saw a 6th-magnitude star in Taurus occulted by Saturn on January 17, 1679—an observation which is unique. Saturn in his short path in Cancer does not come near any naked-eye star. Uranus describes a short retrograde path in Virgo to the N.W. of Spica through a region barren of stars to the naked eye. He rises on the 1st at 5h. 2m. P.M., and souths on the 31st at 8h. 30m. P.M. Neptune is practically invisible. There are no well-marked showers of shooting stars in May. The moon enters her first quarter at 6h. 42m. on the morning of the 8th, is full at the same time on the morning of the 15th, enters her last quarter at 9h. 53m. P.M. on the 21st, and is new at 5h. 20m. P.M. on the 29th. There will be a near approach of the $6\frac{1}{2}$ magnitude star B.A.C. 1835 at 1m. after 11h. on the evening of the 3rd, at an angle of 33° from the vertex; of the $5\frac{1}{2}$ magnitude star 63 Geminorum at 9h. 53m. P.M. on the 5th, at an angle of 32° from the vertex; and of the 6th magnitude star B.A.C. 2788 at 10h. 18m. P.M. on the 6th, at an angle of 207° from the vertex. The $6\frac{1}{2}$ magnitude star 80 Cancri will disappear at 10h. 16m. P.M. on the 7th at an angle of 200° from the vertex, and reappear at 11h. 17m. P.M. the same evening at an angle of 303°. At 1h. 36m. A.M. on the morning of the 8th, the 6th magnitude star 83 Cancri will disappear at an angle of 143°, the star being below the horizon of Greenwich at the time of its reappearance. At 1h. 58m. on the morning of the 11th the 6th magnitude star B.A.C. 3996 will disappear at an angle of 47°, and reappear at an angle of 344° at 2h. 27m. the same morning. The 6th magnitude star 80 Virginis will disappear at 2h. 55m. A.M. on the 13th at an angle of 139°, the star being on the horizon at the time of its reappearance. The 6th magnitude star 0^2 Libræ will disappear at an angle of 105° at 11h. 49m. on the night of the 14th, and reappear at an angle of 219° 45m. after midnight. B.A.C. 5408, a $6\frac{1}{2}$ magnitude star, will reappear at an angle of 206° at 8h. 13m. on the evening of the 15th; this star will not have risen when the disappearance occurs. At 2h. 23m. A.M. on the morning of the 18th the 6th magnitude star B.A.C. 6343 will disappear at an angle of 110°, and reappear at an angle of 248° at 3h. 29m. A.M., while the 6th magnitude star 26 Sagittarii will disappear at an angle of 55° at 4h. 12m. on the same morning, and reappear at an angle of 340° at 4h. 59m. A.M.; both disappearance and reappearance will occur in bright sunlight. At 3m. before midnight on the evening of the 18th the $6\frac{1}{2}$ magnitude star B.A.C. 6699 will reappear at an angle of 263°, the star not having risen when first occulted. On the morning of the 19th the wide pair 53 Sagittarii, and B.A.C.

* Why the Council decided not to print it does not appear.

6727 will disappear at 1h. 5m. and 1h. 14m. at an angle of 9°, and reappear at 1h. 43m. and 1h. 51m. at angles of 309° and 312°.

Our Whist Column.

By W. MONTAGU GATTIE.

ANSWERS TO CORRESPONDENTS.

H. F.—The question is one of etiquette, or perhaps we should say of ethics, rather than of law. We imagine that most people would feel bound, in such a case, to call the adversaries' attention to the omission.

JOHN CADDEL, JUN.—The original caller is at liberty to call "abundance" in trumps, which takes precedence of "abundance" in any other suit.

AMERICAN LEADS.

THE principal charm of whist consists in what has been aptly called "the conversation of the game." The cardinal object of each player being to make the best of *twenty-six* cards, it has become an absolute maxim that partners should endeavour to inform each other by their play where their strength lies, so that the two hands may be combined to the most advantage; and this information is conveyed by means of a system of conventions which has been elaborated by the finest players from the results of their experience. Hence we have certain well-established rules of play which it is essential should be mastered by any one who desires to play whist properly; for, without a competent knowledge of them, it is impossible for him either to fathom his partner's resources or to indicate his own.

Of late years there has been a growing tendency among the authorities to add to, and in some respects to modify, these fundamental conventions. Scarcely had the "call for trumps" become a generally recognised signal in whist circles before it was supplemented by the "echo;"* the lead of the "penultimate," or lowest but one, from a five-card suit made its appearance about the same time, and was soon followed by the "ante-penultimate" lead from suits of six cards; and more recently the apostles of the modern school have promulgated a number of dogmas, of which the most important have been systematised under the title of "American Leads." Each of these innovations has, on its introduction, been subjected to severe criticism, and some of them are still far from obtaining universal approval in high quarters—notably what has been called "the plain-suit echo." But the American leads, at any rate, are now adopted by many good players, and there can be no question that they are often the means of imparting valuable information.

At the same time, the student should understand that it is quite possible to play an intelligent and interesting rubber without the aid of any such supplementary conventions; and indeed they are more likely to confuse than to assist those who are not already conversant with the essential principles of the game. With this preliminary caution, we shall proceed to explain the system of American leads, and briefly to review the arguments which have been adduced in its favour and against it.

One of the most important elements of strength in a hand is the number of cards in its long suit, and it is clearly advantageous that one's partner should have precise information in that particular. For this reason methods have been devised by which a player may, in certain cases, afford such information by his lead. For instance, it is now a well-established rule that a player holding king, queen, knave, and at least *two* others, should open with the knave, whereas with only four cards of the suit he would first lead the king. Again, with ace, queen, knave, and *two* others, it has for some time past been customary to follow ace with knave instead of queen. These and similar conventions are not purely arbitrary, but may be justified theoretically on the ground that the lines of play laid down will frequently prevent a partner from blocking the five-card suit by holding up a winning card in it. Thus, in the first of the above examples, a good partner holding ace and one other in the suit would certainly play ace on knave led, whereas, if the king were led, he would as certainly pass it.

The system of American leads in the first place reduces these isolated conventions to a uniform code, and, secondly, supplements them with further rules, not at present universally adopted, for conveying information both as to the numerical strength of a suit and as to the value of the cards composing it.

* For an explanation of these signals, see KNOWLEDGE for February, p. 95, col. 2.

The system may be reduced to the following three maxims:—

1. When you open a strong suit for which there is no prescribed lead of a high card, lead your *fourth best*.

Ex. From king, 10, 8, 6, the 6 should be led whether the hand contains smaller cards or not.

It will be seen that this rule includes and also extends the practice, already referred to, of leading the penultimate and ante-penultimate cards from suits of five and six cards respectively.

2. When you open a strong suit with a high card and next lead a low card, lead your *original fourth best*.

Ex. From king, queen, 8, 6, with or without smaller cards, the prescribed lead is the king.* If the original leader opens the second round of the suit, he should continue with the 6.

3. When, having opened with a high card, you wish to continue with one of two or more *indifferent* high cards, let your choice be regulated by the number of cards you hold in the suit, as follows:—

(*a*) The first lead being one proper to suits of four cards or more, continue with the higher of *two* indifferent high cards if you opened a suit of four, the lower if you opened a suit of five or more; of *three* indifferent high cards continue with the highest if you opened a suit of four, the intermediate if you opened a suit of five, the lowest if you opened a suit of more than five.

Ex. 1. With queen, knave, 10, and one small one, the knave should follow the queen; with more than one small one the 10 should follow the queen. Another particular case is the lead from ace, queen, knave, &c., already mentioned.

Ex. 2. With king, knave, 10, 9, when the 9 forces out the queen in the first round, the leader should continue with king if the lead was from four cards, with knave if it was from five cards, with 10 if it was from more than five cards.

(*b*) The first lead being one proper only to suits of more than four cards, continue with the higher of two indifferent high cards if you opened a suit of five, the lower if you opened a suit of more than five.

Ex. With king, queen, knave, and two small ones, follow knave with king; with more than two small ones, follow knave with queen.

It is important to observe that the third rule only applies to the case of *indifferent* high cards. Thus, the correct lead from a suit headed by all four honours is king, then knave, irrespective of the length of the suit; so that, at the commencement of the second round, the ace, queen, and knave, are *not* indifferent cards, and it would be misleading to continue with anything but the knave. But, if the original leader should commence a third round, he would show by leading the queen that he had at least one other card of the suit besides the ace, and by leading the ace that he had only the queen remaining.

It should also be borne in mind that the system of American leads is not suitable for adoption at all stages of a hand, or under all circumstances. It is, indeed, only intended to apply to an *original* lead; and, like every other rule for leading, it frequently needs modification as the play proceeds. It may be that a player, obtaining the lead for the first time after several tricks have been played, finds it necessary to treat his long suit as if it were a short one, or not to open it at all; or, again, it may be of the first importance that the adversaries should not be enabled to count his hand, in which case he would of course avoid publishing information that would help them to do so. As a general rule, it is unadvisable to adopt a penultimate or ante-penultimate lead when there is great opposing strength in trumps; but to this, again, there are exceptions; and in all such matters the player must be guided by his own judgment.

From the three maxims above enunciated may be deduced at once the following corollaries:—

1. When a low card is led originally the leader holds three of the suit higher than the card led.

2. When a high card is led originally, and then a low one, the leader still holds two cards higher than the second card led.

3. When a high card is led originally, and is followed by a high card, the leader can in many cases indicate precisely his numerical strength in the suit.

This code includes all possible cases of leads from strong suits; and thus a uniformity of practice is attained which, coupled with the fact that valuable information may sometimes be afforded in cases not provided for by generally accepted canons, is to many players irresistibly attractive.

(*To be continued.*)

* "Cavendish" now advocates leading the queen from king, queen, and three or more others, but the suggestion has not met with much favour so far. Of course this question does not affect the principle we are illustrating; for, whichever high card be led, the original fourth best should follow.

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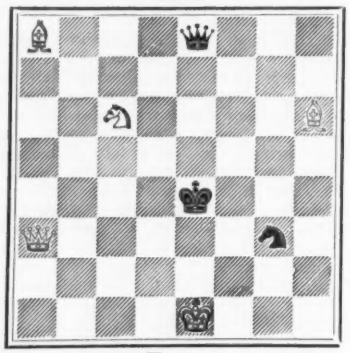
Our Chess Column.

[Our Chess column has been placed in the hands of Mr. R. F. Fenton during the absence of Mr. Gunsberg, who has gone to play at the International Chess Tournament in New York. Mr. Gunsberg will probably return by the middle of May.—Editor.]

PROBLEM BY M. GROSDEMANGE.

Black.



White.

White to play and mate in two moves.
White, 5 pieces; Black, 3 pieces.

THE NEW YORK INTERNATIONAL CHESS CONGRESS.

No event in chess circles has been looked forward to with greater interest than the meeting of the sixth American Chess Congress in New York. The Tournament is open to all the world, and most of the famous masters are combatants in the lists. There are twenty competitors, and each player has to play two games with every other player. At the end of the first round of 19 games the principal scores were:—1st, Herr Weiss, Vienna, 15½ games; 2cd, J. H. Blackburne, 15; 3rd, I. Gunsberg, 14; and M. Tschigorin, followed by A. Burn, 13, S. Lipschutz, 12, and J. Mason, with M. Taubenhaus, 10 games each.

There are two notable absentees in the list of celebrated masters, Herr Wilhelmn Steinitz and Captain G. Mackenzie, the winner of three American Congresses, the former great expert, to whom by common consent is adjudged the sovereignty of our noble game, is thought to be reserving his powers for the final match for the championship of the world which is to follow upon the termination of the Congress.

Captain Mackenzie would have been a certain competitor but at the last moment found himself attacked by illness, of a nature which precluded him from taking part in the exhausting struggle for supremacy, but he is now much better, and is often present as a spectator.

The first American Congress took place in 1857, and will be for ever memorable in the history of the most intellectual of games, as introducing to the chess world that unrivalled genius, Paul Morphy. Although at that time a lad but of nineteen years, he speedily proved himself immeasurably superior to all the players of the New World, and, crossing the ocean, engaged, with uninterrupted success, the best masters of Europe. That great triumvirate of players, Andersen, Harwitz, and Lowenthal, all engaged in matches with the new prodigy, only to be easily vanquished, and when he recrossed the sea it was with an unbeaten record.

The second tournament took place in 1871, and was won by Captain Mackenzie, who had left this country some time previously to reside in the United States. The third tournament, held in 1874, was also won by him, the fourth was carried off by James Mason, whilst the fifth fell again to the redoubtable captain. The conditions of the present tournament are that each player shall contest two games with every other player, and that the seven prizes of the respective values of 200*l*., 150*l*., 120*l*., 100*l*., 80*l*., 60*l*., and 40*l*., shall be awarded according to the total number of games won or lost by each player at the close of the tournament.

The time limit was fixed at fifteen moves per hour, and in the first round play will take place every day except Sunday, but in the second round Thursday also will be excepted. It was also made a stipulation that in this round the first draw shall not count, but a second game shall be played, and, if this too be drawn, it shall count one-half for each player.

We append the game played between Mr. I. Gunsberg and Mr. Max Judd, the champion player of New Orleans, in the first round of the tournament:—

FRENCH DEFENCE.

White. I. Gunsberg.	Black. Max Judd.	White. I. Gunsberg.	Black. Max Judd.
1. P to K4	P to K3	17. B to Kt3	P to Kt5
2. P to Q4	P to Q4	18. P to QB4	B to Bsq
3. QKt to B3	KKt to B3	19. P to B5	Q to B2
4. B to KKt5	B to K2	20. P to B4	P to B4
5. B × Kt	B × B	21. Kt to Kt3	B × Kt (*d*)
6. Kt to B3	P × P	22. RP × B	P to Kt3
7. Kt × KP	Castles	23. Kt to Bsq	B to R3
8. P to B3	P to QKt3	24. Kt to Q2	Q to B2
9. B to Q3	R to Ksq	25. Kt to B3	QR to Qsq
10. P to KR4 (*a*)	Kt to Q2	26. R to R6	B to Kt4
11. KKt to Kt5	Kt to Bsq	27. P to Kt4 (*e*)	K to Rsq (*f*)
12. Q to B3 (*b*)	R to Ktsq	28. Kt to K5 (*g*)	Q to QB2
13. Castles (QR)	B to Kt2	29. QR to Rsq	R to K2
14. Q to K3	B to K2	30. Kt × P (ch)	Kt × Kt
15. B to Kt5 (*c*)	P to QB3	31. R × Kt	R to Q4
16. B to R4	P to QKt4	32. KR to R6	Resigns (*h*)

NOTES.

(*a*) Played with characteristic boldness; but he has also an eye to Castling on the Queen's side.

(*b*) Threatening to win the QR.

(*c*) In order that Black by playing P to QB3, may block the operations of his own B.

(*d*) The opening of White's R's file was injudicious, and proved in the end prejudicial to Mr. Judd's game.

(*e*) A very clever mode of proceeding with the attack.

(*f*) It is evident he could not advantageously capture the Pawn.

(*g*) This Knight's moves have been admirably contrived, and he now assists materially in the final onslaught.

(*h*) The on coming of the Knight's Pawn next move is fatal.

The annual meeting of the City of London Chess Club was held on the 29th ult., when Mr. J. H. Clarke was elected the new president. The club has had a very prosperous year under the auspices of the outgoing president, Mr. Anger, himself one of the strongest amateurs of the day. The important match between the City of London and the St. George's Chess Clubs was played on the 3rd inst., fifteen players aside, at the rooms of the former, and in the end was won by the City with 9½ to 5½ games. This is the sixth match between these powerful clubs, and of these the City has won 4 and the St. George's Club 2. A match was also played between the British Chess Club and the St. George's Chess Club, and, after a stubborn fight, resulted in a victory for the former by 7½ to 5½ games. This is the second match between these clubs, and both have been won by the British.

ANSWERS TO CORRESPONDENTS.

W. Cordwell.—There are several good works devoted to the chess openings alone. The best are Cook's Synopsis, 1882; American Supplement to the Synopsis, 1885; and "Chess Openings, Ancient and Modern," by E. Freeborough and the Rev. C. E. Ranken (London: Trübner & Co.). The prices of all are very moderate; but you had better write to W. W. Morgan, chess publisher, 17 Medina Road, Holloway, London, N., who will give you more detailed information with respect to the work you mention. I believe it is a chart only of the openings, suitable for hanging up in clubs.

T.—Thanks for your note. There are, of course, positions where the Rook wins against the Bishop; but in nearly all cases the game is easily drawn.

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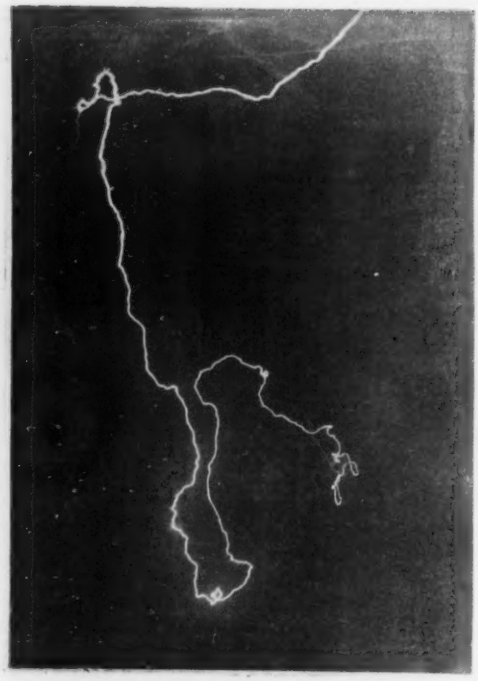
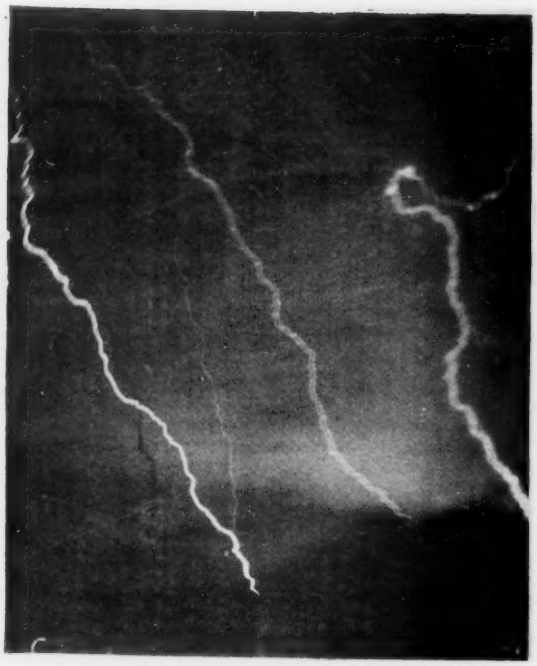
XUM

1. Broad Ribbon Lightning.—Taken at Battersea, on August 17th, 1887, by G. J. Ninnes.

2. Curtain-like Lightning.—Taken from Westbourne Grove, London, on August 17th, 1887, by E. S. Shepherd.

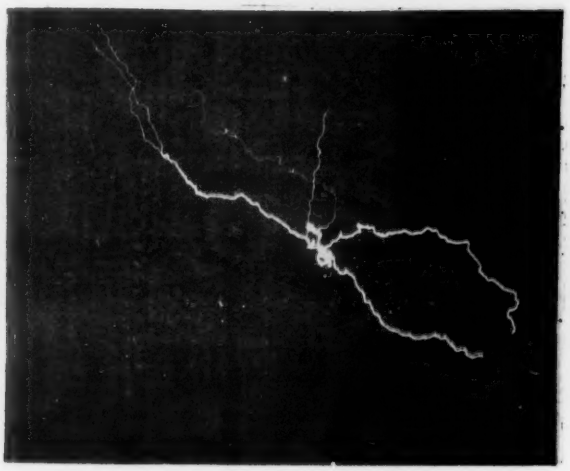
3. Meandering Lightning.—Taken at Brixton, on August 17th, 1887, by J. Gray.





4. This Photograph—in addition to three bright flashes—shews a *dark* flash. It was taken from Westbourne Grove, London, on August 17th, 1887, by E. S. Shepherd.

5. Meandering Lightning.—Taken at Putney on August 17th, 1887, by A. W. Bates.



6. Ribbon and Knotted Lightning.—Taken at Clapham on August 17th, 1887, by J. Guardia.

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LIGHTNING PHOTOGRAPHS.